AN ACCOUNT

OF

THE LATE IMPROVEMENTS

IN

GALVANISM,

WITH A SERIES OF CURIOUS AND INTERESTING

EXPERIMENTS

PERFORMED

BEFORE THE COMMISSIONERS OF THE FRENCH NATIONAL INSTITUTE,

AND REPEATED LATELY IN THE

ANATOMICAL THEATRES OF LONDON.

BY JOHN ALDINI,

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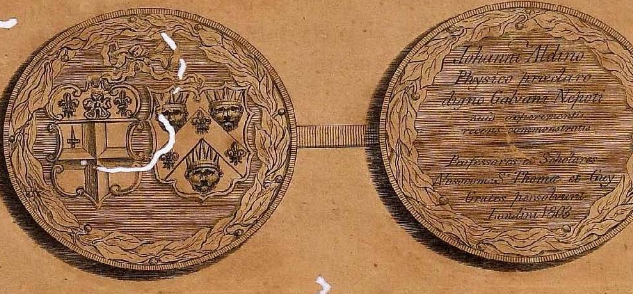
TO WHICH IS ADDED,

AN APPENDIX,

CONTAINING THE AUTHOR'S EXPERIMENTS
ON THE BODY OF A MALEFACTOR EXECUTED AT NEWGATE.

&c. &c.

ILLUSTRATED WITH ENGRAVINGS.



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EDITOR'S PREFACE.

FEW discoveries in modern times have excited so much curiosity as that of Galvanism. Ever since it was first made known by its celebrated Author, it has engaged the attention of the most eminent philoſophers in Europe; and various researches have been undertaken to ascertain the principles on which it depends, and the laws to which it is subject.

Though some of its singular properties are fully established, it must be allowed that the discovery is still in its infancy; but enough of it is known to prove its importance, and to induce philosophers to continue their researches, which there is every reason to suppose may lead to some very curious results.

The experiments, indeed, which have already been made, seem to indicate that it may open a new field in the healing art; and it appears by a late report preſented to the Class of the Exact Sciences of the Academy

 of

of Turin, that the medical application of it has been attended with the most beneficial effects in a case of confirmed hydrophobia.

While Galvanism, independently of other advantages, holds out such hopes of utility in regard to objects so interesting to mankind; a work containing a full account of the late improvements which have been made in it, illustrated by a complete course of experiments, cannot fail of being acceptable to the public in general, and in particular to medical men, to whose department, in one point of view, it more essentially belongs.

When Professor Aldini left this country, the manuscript, written in French, together with two printed Latin Dissertations, was put into the Editor's hands, in order that they might be prepared for the press. A translation of these forms the principal part of the work: and an Appendix has been added, containing the author's experiments on the body of a malefactor executed at Newgate; experiments of a similar kind on the bodies of three criminals decapitated at Bologna; and an experiment lately made at Calais, which

seems to show that Galvanism is susceptible of being conveyed to a very considerable distance through the water of the sea.

The Editor thinks it necessary to observe, that the principal experiments, of which an account is given in this work, are illustrated by proper engravings, and that the title page is embellished with a representation of the gold medal presented to the Author, as a mark of their respect, by the medical professors and pupils of Guy's and St. Thomas's Hospitals.

LONDON,
May 12th, 1803.

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AN ACCOUNT

OF THE LATE

IMPROVEMENTS IN GALVANISM.

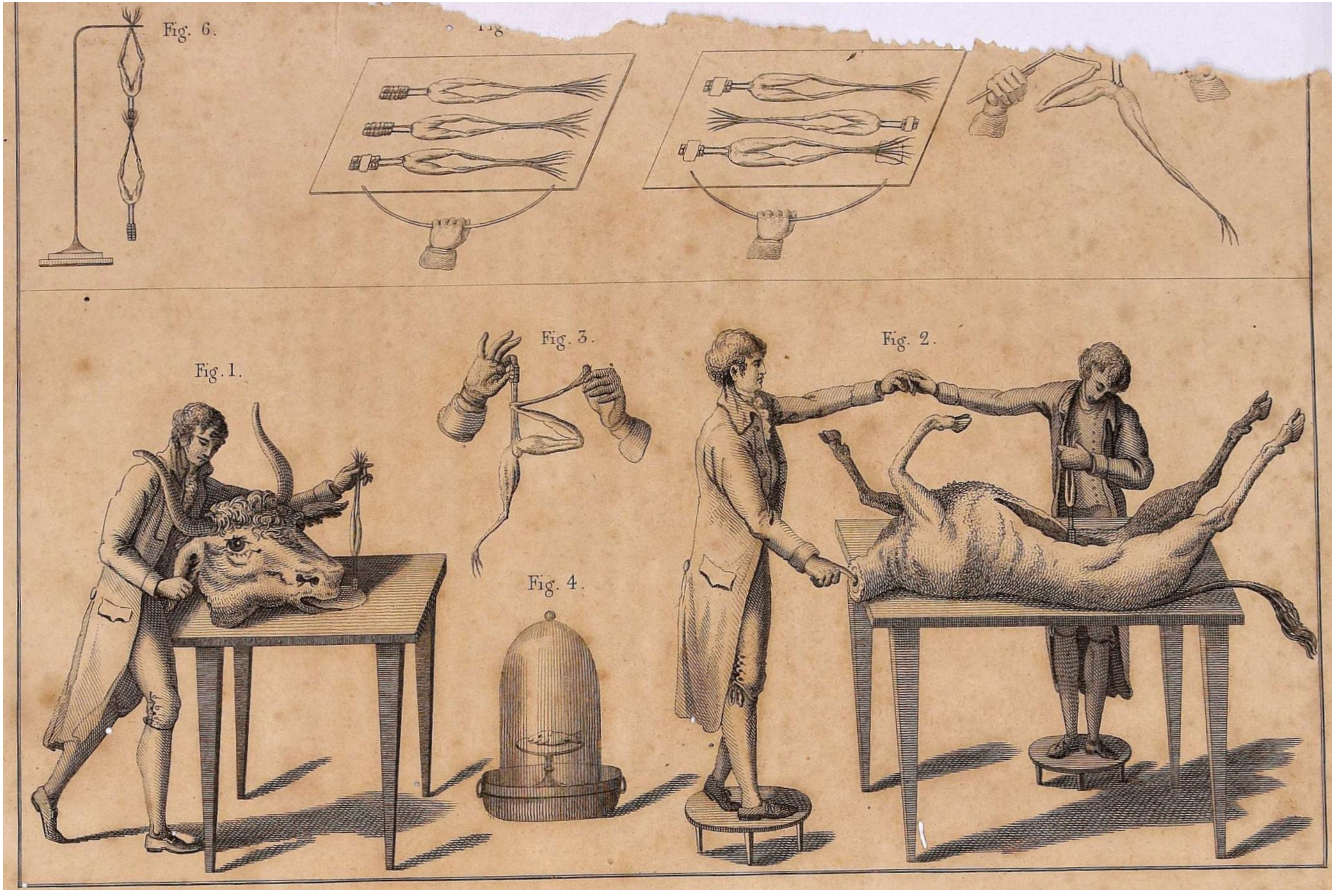
A JUST tribute of applause has been bestowed on the celebrated Professor Volta for his late discovery; and I have no desire to deprive him of any part of that honour to which he is so justly entitled; but I am far from entertaining an idea that we ought, on this account, to neglect the first labours of Galvani. Though these two philosophers pursued different routes, they concurred to throw considerable light on the same points of science; and the question now is, to determine which of them deduced the most just consequences from the facts he observed; and then to ascertain whether the facts established by Galvani lead to the theory of Volta, or whether those discovered by Volta are connected with the theory of Galvani. For my part, I am of opinion that these two theories may serve in an eminent degree to illustrate each other.

Last year Professor Volta announced to the public the action of the metallic pile. I here propose to exhibit, accord-

ing to the principles of Professor Galvani, the action of the animal pile.

Such is the plan I have conceived in order to reconcile the systems of these two illustrious philosophers: it forms the object of the present work, which is divided into three parts. In the first I shall exhibit the action of Galvanism independently of metals, and explain some of its general properties. The second will contain experiments on the power of Galvanism to excite the vital forces. In the third I shall propose some useful applications of it to medicine, and explain the principles on which the new medical administration of Galvanism is founded. To render the work as methodical as possible, I have endeavoured to arrange the experiments in such a manner that they may serve as proofs to a series of general propositions, which, it is hoped, will be of use to physiology and to the doctrine of the animal economy.

PART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PART THE FIRST.

OF THE NATURE AND GENERAL PROPERTIES OF GALVANISM.

PROPOSITION I.

Muscular contractions are excited by the development of a fluid in the animal machine, which is conducted from the nerves to the muscles without the concurrence or action of metals.

EXPERIMENT I.

HAVING provided the head of an ox, recently killed, I thrust a finger of one of my hands, moistened with salt water, into one of the ears (Plate I. fig. 1.), at the same time that I held a prepared frog in the other hand, in such a manner that its spinal marrow touched the upper part of the tongue. When this arrangement was made, strong convulsions were observed in the frog; but on separating the arc all the contractions ceased.

This experiment will succeed still better if the arc be conveyed from the tongue of the ox to the spinal marrow of the frog. This method was found to be exceedingly convenient for trying the effect of Galvanism on several calves.

EXPERIMENT I.

Having provided the trunk of a calf, I conveyed the arc from the muscles of the abdomen to the spinal marrow of a frog, prepared and arranged in the usual manner. The frog seemed much affected, and the contractions were exceedingly violent when the arc was composed of a chain of different persons, united together by the hands moistened with salt water.

EXPERIMENT III.

I connected, by means of one chain of moisture, the heads of two or three calves, and observed that by this combination the force of the Galvanism was exerted with more energy: a frog, which was not affected by touching one head, experienced violent contractions when applied to a series of several heads connected together.

EXPERIMENT IV.

I think it proper here to mention a very curious observation which I made lately at Paris, in company with professor Huzzard, and in the presence of the Commissioners of the National Institute. On applying the spinal marrow of a prepared frog to the cervical muscles of a horse's head, separated from the body, no muscular convulsions took place; but if, at the same time, another person touched with his hand, moistened

moistened by a solution of muriate of soda, the spinal marrow of the horse, convulsions were always produced in the frog, though there was no communication between the persons, except that formed by a floor on which they stood.

PRO-

PROPOSITION II.

The Galvanism excited, in the preceding experiments, is not owing to the communication nor to the transfusion of the general electricity, but to an electricity peculiar to animals, which acts a very distinguished part in the animal economy.

EXPERIMENT I.

Having placed the trunk of a calf (Plate I. fig. 2.) on an insulated table, I made a longitudinal incision in the breast, in order to obtain a long series of muscles uncovered. I then arranged two insulated persons in such a manner that the one with a finger, moistened by salt water, touched the spinal marrow of the calf, while the other applied the spinal marrow of a frog to the muscles of the trunk. Every time this arc was formed, muscular contractions were produced in the frog. When the two persons let go each other's hands, the contractions ceased. I repeated this experiment, with the same success, on the insulated head of an ox, conveying the arc from the spinal marrow of the frog to the tongue. Frogs were as violently affected when the experiment was made with the insulated trunks of different kinds of birds.

This experiment, in my opinion, affords a decisive proof that the Galvanic fluid is peculiar to the animal machine, independently of the influence of metals, or of any other foreign

foreign cause. In these experiments, indeed, we have some animal machines, so combined that the result is strong contractions in the frog. All the bodies were insulated; and, therefore, it cannot be supposed that the contractions were occasioned by the direct influence of that general principle, which pervades every body in nature. Hence it is evident, whether it be ascribed to the action of the animal chain, formed by the arms of the persons, or to the animal pile, formed by the trunk of the calf, that we shall still be obliged to acknowledge the action of a principle which belongs to the organization of the animal machine, without having any dependence on metals.

To prove in the animal body the existence of a principle which philosophers can by certain means excite and direct at pleasure in their experiments, is a matter of the greatest importance; though the manner in which it is put in action by nature, however wonderful, is unknown to us. Here then we have developed a very energetic fluid, capable of transmission, and deriving its origin from the action of the animal forces; since the parts of bodies separated from the common reservoir of general electricity have still of themselves the faculty of reproducing it, and of causing it to circulate in a manner proper for exciting muscular contractions.

PRO-

PROPOSITION III.

Galvanism develops itself in a powerful manner, independently of metals, by means of the human animal machine.

EXPERIMENT I.

If you hold in your hand, moistened with salt water, the muscles of a prepared frog, and apply the crural nerves to the tip of your tongue, you will immediately see violent contractions produced in the frog. All suspicion of any stimulant exerting an action in this case, may be removed by repeating the experiment with the frog held in the dry hand: the muscular contractions will then cease, unless the action of Galvanism in the frog, or in the animal machine, be uncommonly powerful; in which case contractions may be produced without establishing an arc from the nerves to the muscles.

EXPERIMENT II.

I held the muscles of a prepared frog in one of my hands, moistened by salt water, and brought a finger of the other hand, well moistened, near to the crural nerves. When the frog possessed a great deal of vitality the crural nerves gradually approached my hand, and strong contractions took place

place at the point of contact. This experiment proves the existence of a very remarkable kind of attraction, observed not only by myself, but also by those whom I requested to repeat the experiment.

EXPERIMENT III.

The above experiment requires great precision in the preparation, and a considerable degree of vital power in the frog. I have been informed by Professor Fontana, in a letter lately received from him, that this phænomenon depends on very delicate circumstances, which he proposes to explain. He assures me, at the same time, that he has twice seen the nerve attracted, in this manner, by the muscle. Being desirous to render this phænomenon more evident, I formed the arc, by applying one of my hands to the spinal marrow of a warm-blooded animal, while I held a frog in the other, in such a manner that the crural nerves were brought very near to the abdominal muscles. By this arrangement the attraction of the nerves of the frog became very sensible. I performed this experiment for the first time, at Oxford, before Sir Christopher Pegge and Dr. Bancroft, and repeated it in the anatomical theatres of St. Thomas's and Guy's hospitals.

EXPERIMENT IV.

I made the same observations on the body of a man as I had before made on the head and trunk of an ox. Having obtained the body of an executed criminal, I formed an arc from the spinal marrow to the muscles, a prepared frog being placed between, and always obtained strong contractions without the aid of the pile, and without the least influence from metals. I obtained the same result, in a certain degree, from the bodies of men who had died a natural death.

EXPERIMENT V.

Let four or more persons hold each other by the hands, moistened by a solution of muriate of soda, so as to form a long animal chain. If the first hold in his hand the muscles of a prepared frog; and if the last, at the other end of the chain, touch the spinal marrow or the crural nerves, contractions will be produced: if the animal chain be broken, the contractions will immediately cease. I performed this experiment, making the animal chain to consist of two persons, before the Galvanic Society at Paris, and in Mr. Wilson's anatomical theatre, Windmill-street.

PROPOSITION IV.

Muscular contractions can be excited, under certain conditions, without establishing a continued arc from the nerves to the muscles.

EXPERIMENT.

Having obtained the body of an executed criminal, I caused the biceps muscle to be laid bare, and brought near to it the spinal marrow of a prepared frog. By these means contractions were produced in it much stronger than I had ever obtained in warm-blooded animals. I repeated the experiment, being myself insulated, and observed no signs of contraction. The same phænomena were exhibited with the head of an ox, which possessed an extraordinary degree of vitality.

PROPOSITION V.

The effects of Galvanism, in the preceding experiments, do not depend on the action of any stimulant, which occurs in performing the experiments, and ought not to be confounded with the effects of that action.

EXPERIMENT I.

In the experiment of the frog applied to the uncovered biceps muscle of the body of the malefactor, if any other body be made to touch the frog it will remain motionless. This proves that the contractions produced in the frog do not arise from the impulse of the mere contact of the spinal marrow with the muscle of the human animal machine.

EXPERIMENT II.

To remove still further all suspicion of the action of stimulants, in the preceding experiments, I prepared two frogs, and connected the extremities of one with the spinal marrow of the other. I then held in my hand the extremities of

of one of the frogs, and applied the spinal marrow of the other to the uncovered muscles of the head of an ox, which possessed a great degree of vitality. By these means contractions were produced in both the frogs. It is evident, in this experiment, that the force of the stimulant, if there were any, might act on the second frog, but not on the first.

PROPOSITION VI.

Galvanism is excited in the animal machine without any intermediate body, and merely by the application of the nerves to the muscles.

Several philosophers have endeavoured to obtain this interesting result. Professor Volta, in a letter which he addressed to me, in Brugnatelli's Journal, observed, "that various parts of animals can excite Galvanism, independently of metals." Galvani, a short time before his death, proposed two ingenious methods of obtaining this result, and gave me a description of them. This, however, has not been able to destroy the incredulity of some philosophers, who hitherto have confounded Galvanism with metallic electricity, under an idea that all contractions proceed from irritation, produced by the action of metals. For this reason I have, with confidence, announced my method, which enables any one to observe this important result.

EXPERIMENT I.

Having prepared a frog in the usual manner, I hold the spinal marrow in one hand (Plate I. fig. 3.), and with the other form an angle with the leg and foot, in such a manner that the muscles of the leg touch the crural nerves. On this contact

contact strong contractions, forming a real electrico-animal alarum (*carillon*), which continue longer or shorter according to the degree of vitality, are produced in the extremity left to itself. In this experiment, as well as in the following, it is necessary that the frogs should be strong and full of vitality, and that the muscles should not be overcharged with blood.

EXPERIMENT II.

By observing the directions already given, very strong convulsions will be obtained; but they must not be ascribed to the impulse produced by bringing the nerve into contact with the muscle. If the experiment be repeated, covering the muscle, at the place of contact, with a non-conducting substance, the contractions will entirely cease; but they will be re-produced as soon as the nerve is made to touch the muscular substance. In performing this experiment, in public, I obtained several times more than two hundred successive contractions; but this was never the case when I formed the same contact with the muscle by means of a conducting substance, and even with a plate of metal.

To ensure the success of this interesting experiment, the nerves must be prepared as speedily as possible, by disengaging them from every foreign substance. It will be proper also to apply the nerves not to one but to several points of the muscle, throughout its whole length. It is observed, that the contact of the nerves with the tendinous parts which communicate with the muscles, often serves to

increase the muscular contractions. I performed the above experiment before several able professors, among whom were the celebrated Brugnatelli and Carcano, who, with that modesty peculiar to them, made several ingenious observations on the precision which might be given to it. Professor Brugnatelli was apprehensive that, as I had accidentally touched some metals before I performed the experiment, metallic particles might have adhered to my fingers, and thus have served, in some measure, as invisible arming, sufficient of itself to excite muscular contractions. This suspicion, however, I removed, by immersing my hands in water, to detach every foreign substance. He then observed that animal moisture, independently of the circulation of the Galvanic fluid from the nerves to the muscles, might also excite muscular contractions; and he requested that the crural nerves might be washed in common water. This was accordingly done; and the humidity of the nerves being thus externally removed, very strong contractions were still produced, as the professor found, to his full conviction, on repeating the experiment himself several times *.

EXPE-

* It may not be improper here to observe, that my method of exciting muscular contractions, without metals, is very different from that proposed by others. I do not know that convulsions have ever been obtained in cold-blooded animals by means of warm-blooded. From observations I have made, I flatter myself with the hope of being able to obtain contractions without metals, even in the muscles of warm-blooded-animals. But to ensure the certainty of this method would require long practice, and a preparation attended with considerable difficulty. I however propose to attempt it on my return to Italy. Some philosophers, indeed,

EXPERIMENT III.

The Commissioners of the French National Institute remarked, that, in order to give the greatest precision possible to these experiments, it would be necessary to insulate entirely the nervous and muscular systems. For this purpose, I applied these parts to each other by means of glass rods, and each time they were brought into contact I obtained muscular contractions. The case was the same when an animal arc was applied to two insulated frogs: contractions were produced in them both. The apparatus employed for this purpose may be seen in Plate I. fig. 5 and 6.

indeed, had conceived the idea of producing contractions in a frog without metals; and ingenious methods proposed by my uncle Galvani induced me to pay attention to the subject, in order that I might attain to greater simplicity. He made me sensible of the importance of th experiment, and therefore I was long ago inspired with a desire of discovering that interesting process. It will be seen in the *Opuscoli of Milan*, that I shewed publicly, to the Institute of Bologna, contractions in a frog without the aid of metals, so far back as the year 1794. The experiment, as described in a memoir addressed to M. Amorotti, is as follows: "I immersed a prepared frog in a strong solution of muriate of soda. I then took it from the solution, and, holding one extremity of it in my hand, I suffered the other to hang freely down. While in this situation, I raised up the nerves with a small glass rod, in such a manner that they did not touch the muscles. I then suddenly removed the glass rod, and every time that the spinal marrow and nerves touched the muscular parts, contractions were excited. Any idea of a stimulus arising either from the action of the salt, or from the impulse produced by the fall of the nerves, may be easily removed. Nothing will be necessary but to apply the same nerves to the muscles of another prepared frog, not in a Galvanic circle; for, in this case, neither the salt, nor the impulse even if more violent, will produce muscular motion.

EXPERI-

EXPERIMENT IV.

Having prepared a frog according to the usual method, I cut one of its crural nerves in such a manner that the trunk was united to the spinal marrow by means of the other nerve, which remained uncut, and also by a blood-vessel contiguous and parallel to the cut nerve. I then repeated the above experiment; and, though only one nerve was in contact with the muscles, I obtained the same results.

EXPERIMENT V.

A ligature was placed loosely around the middle of the crural nerves, and one of these nerves at the ligature applied to the corresponding muscles: strong contractions ensued; which, however, did not take place, when the ligature was drawn tight, at the insertion of the nerves into the muscles of the thigh.

PRO-

PROPOSITION VII.

The heterogeneity of metals contributes, in a great degree, to excite muscular contractions with more facility, but is not absolutely necessary to their production.

This proposition I could demonstrate in a direct manner, by means of experiments, which I published formerly, on the contractions excited by very pure mercury, and which were repeated, in different ways, by the celebrated Humboldt. I am, however, happy to have an opportunity of examining the influençe of arming with heterogeneous substances; and I shall endeavour to prove that it cannot, of itself, produce the effect of muscular contractions.

EXPERIMENT I.

If several prepared frogs, ten or more for example, be placed on a table (Plate I. fig. 7.), and arranged parallel to each other, in such a manner that the whole system of the nerves shall be at one end, and that of the muscles at the other,—on applying two armatures and a metallic arc to the first of these frogs, muscular convulsions will be immediately excited, not only in the first frog, but in all the rest.

EXPERIMENT II.

If the experiment be repeated with the frogs arranged in such a manner that the spinal marrow and muscles are not each at one end (Plate I. fig. 8.), but disposed alternately so that the spinal marrow of one touches sometimes the muscles of another, or *vice versa*, convulsions will then be produced only in some of the frogs, and not in the whole series. This experiment proves that the effect does not, in any manner, depend on the action of metals; because metallic electricity in the first experiment ought to exercise an action only on the first frog, and not on the rest; and, in the second, ought to cause them all to move together, or to leave them motionless.

I shall now proceed to those experiments which appear to be best calculated to support the opinion of the great analogy between electricity and Galvanism.

PRO-

PROPOSITION VIII.

The Leyden flask, the Voltaic pile, and animal substances, have the faculty of absorbing principles from the atmospheric air in an insulated plenum.

EXPERIMENT I.

By means of a metallic point, I electrified the interior side of a glass jar, which I inverted and placed on a plate of metal, so as to form an insulated plenum. In a little time, I saw the water rise in the glass several lines; and I then flattered myself with the hopes of obtaining some remarkable effects by another method.

EXPERIMENT II.

I provided for this experiment a Leyden flask, seven inches in height and about three in diameter, coated in the usual manner with tin foil: the exterior end of the wire terminated in a sharp point, so that the electric fluid which escaped from it could easily combine with the principles of the atmospheric air, with which it had a greater affinity. I then electrified the jar, and covered it with a glass receiver of such a size that its electricity could not be weakened

weakened by the sides of the latter. I thus formed an insulated plenum, and at the end of half an hour I saw the water ascend in the receiver in a very sensible manner.

EXPERIMENT III.

Having made the wire to terminate, not in a point, but in a metallic knob, as usual, I again charged the jar, and having placed it under a common receiver, at the end of about half an hour I found that the elevation of the water was much greater. To remove every suspicion that this might arise from the water employed in the preceding experiment, to insulate the plenum, I substituted mercury in its stead; and though the elevations were less, they were, however, analogous to those which had been observed a little before with water. By repeating this experiment with a similar jar, not electrified, one may be easily convinced, that the elevation of the water in the bell ought not to be ascribed to a difference in the temperature of the air within it.

EXPERIMENT IV.

I placed under a bell-glass, forming an insulated plenum, a pile consisting of fifty plates of silver and zinc. Next morning I observed that the water had risen some inches, indicating that a great absorption of air had taken place. Having then introduced a taper into the receiver, it was imme-

immediately extinguished. The pile, without being arranged anew, was placed under the same receiver; and on forming an insulated plenum, I observed, after twenty-four hours had elapsed, a sensible absorption of air. A taper was then introduced, and I obtained the same result. I replaced the pile under the receiver, and found, on the third and following days, that the pile retained its moisture, so that till the tenth day it gave analogous results. I repeated the same experiment with oxygen gas, and found, six days after, that the water in the bell had risen a foot.

EXPERIMENT V.

The same results may be obtained without employing large piles and large receivers. In general, it will be sufficient to arrange, in alternate strata, some plates of heterogeneous metals. If two plates of copper and zinc be placed under a bell an inch and a half in diameter, and three inches in height, and if an insulated plenum be then formed, two days after the water will have risen about half an inch. Having repeated the experiment with different metals, I found that a greater or less absorption of air had taken place, according to the difference of their nature and combination. This inspired me with the idea of making a series of experiments with different metals; and I hope to be able, at some future period, to form a table of the different heights of the fluid, which may serve to determine how far they are respec-

respectively susceptible of oxidation. However, to ascertain the oxidation of metals with precision, pieces of coin mixed with alloy ought not to be employed. Pure metals, formed into small piles, must be subjected to observation, and ought to be placed under equal bells, at the same temperature as that of the atmosphere. Until it be proved that the absorption of oxygen in the above experiments is merely a chemical effect, altogether unconnected with the action of Galvanism, I think I may be allowed to avail myself of it to prove the proposed analogy.

EXPERIMENT VI.

The ingenious theory of Girtanner, who ascribes the cause of muscular contractions to oxygen, the curious experiments by which Professor Humboldt revives the muscular force, by means of oxygenated muriatic acid, and those made by the celebrated Fourcroy on the same subject, induced me to examine the effect resulting from a combination of oxygen with muscular fibres, in a state of the greatest vitality. For this purpose, I adapted to a bell-glass a bent metallic wire, from which were suspended fourteen frogs, prepared with the utmost dispatch, and almost at the same instant, by myself and several of my pupils; and having formed an insulated plenum, I found, at the end of twenty-four hours, that the water had risen in the bell to the height of about half an inch.

EXPERIMENT VII.

I repeated this experiment, with the same success, on warm-blooded animals. I provided, for that purpose, the extremities of different pullets from which the crural nerves had been previously separated, and found that the elevations of the water, in the insulated plenum, were much less when I employed the fibres of these animals after their vitality had been weakened.

EXPERIMENT VIII.

Having obtained the bodies of some executed criminals, I exposed to the action of an insulated plenum the nervous and muscular fibres, and the substance of the brain. The elevations of the water were remarkable, in consequence of the different substances subjected to experiment, which, according to their different characters, exercised a different action on the oxygen. This fact ought to induce physiologists to undertake experiments of a similar kind with other gases, to enable them to determine the strength of the affinity exerted by animal substances to combine with oxygen.

EXPERIMENT IX.

As fishes, and in particular the torpedo, furnish a large quantity of animal or Galvanic electricity, I was inclined to

think they would exhibit the before-mentioned effects in a very striking manner in an insulated plenum. I mentioned to Professor Mojon of Genoa the experiment I proposed to make; and, in a letter which I lately received from him, he informed me of the result, as follows:

"I took a strong torpedo, and, as soon as it was dead, armed its nerves with the usual armature. Having then placed it on an insulating stool, a little elevated above water, I covered it with a bell-glass the content of which was equal to 432 cubic inches. At the end of some hours I observed, with great surprise, that the water under the insulated plenum began to rise progressively during about ten hours; and at the end of forty-eight I found that it had risen an inch; so that it occupied a ninth part of the capacity of the bell, that is to say, forty-eight cubic inches. I analysed the remaining air, and found that the bell contained no more than 80 cubic inches of oxygen gas, and 324 of azotic gas; and that, during the above period, more than two-fifths of the oxygen gas contained in the bell had been absorbed."

I propose going to the sea-coast, in order that I may repeat the experiment on the torpedo without any armature; and I shall embrace that opportunity of making various researches in regard to the new theory of Galvanism. I think it necessary, in general, to submit to new experiments the different animal parts immersed in the different aëriform fluids, fixing their various combinations according to the degrees of Galvanic force which they may possess.

PRO-

PROPOSITION IX.

Flame prevents the action of the Leyden flask, as well as that of the pile, and also muscular contractions.

EXPERIMENT I.

I placed a lighted taper on an insulating stool; and having made the wire, proceeding from the interior coating of a charged Leyden flask, to pass through the flame, I found that, without forming an arc, it lost a portion of its electricity. If the experiment be repeated in such a manner that the flame makes a part of the arc between the two coatings, the flask is entirely discharged, without the arms of the person who forms part of the arc experiencing the least shock.

EXPERIMENT II.

I adapted to the summit of the pile a circular brass vessel, containing spirit of wine. By these means the pile was made to terminate in a strong flame, to which I applied a metallic conductor, while with the other hand I touched the bottom of the pile. The Galvanic fluid still withstood my efforts; and the case was the same when I substituted for the spirit of wine the flame of a common candle. It is

proper here to remark, that the flame did not lessen the action of the Galvanism when the conductor, instead of being applied to the flame, was applied to the plate at the summit of the pile.

EXPERIMENT III.

I have already proved by a series of experiments, addressed to C. Lacepede, that flame made to form part of the arc applied to the nerves and muscles of a frog, prevents muscular contractions. I repeated the experiment, with the same result, on several warm-blooded animals. I observed that the flame interposed in the arc, which touched the back and belly of the torpedo, prevented the electric shocks.

PRO-

PROPOSITION X.

Certain fluids, applied to the whole surface of the pile, or of animal parts, do not prevent the action of Galvanism.

EXPERIMENT I.

Two years ago, I made various experiments on this subject at Florence, with the celebrated Fontana; and we found that a pile, composed of a hundred plates of zinc and silver, after being immersed some time in common water, still exercised a very strong action. Professor Fontana informs me, in a letter, that he has performed the same experiment several ways, and always with the same success.

EXPERIMENT II.

Being desirous to examine the nature of the element inhabited by the numerous family of fishes, which are also subject to the influence of the Galvanic processes, I filled with sea-water thirty earthen vessels; and having formed a communication between them, by means of heterogeneous arcs, composed of brass and zinc, I obtained a shock, which appeared to me stronger than that obtained with artificial salt water. By establishing an arc with only five of these vessels,

sels, the action was very sensible. A pile composed of pieces of pasteboard, moistened with sea-water, and entirely immersed in the same water, gave, when tried, very strong shocks.

EXPERIMENT III.

I was able to prove the action of the Galvanic pile and of metals under water, by the following simple experiment: I placed a plate of zinc at the bottom of a vessel filled with salt water, (Plate II. fig. 6.) A person then brought the spinal marrow of a frog into contact with the surface of the salt water; and another person, absolutely insulated, touched with a silvered copper wire the plate of zinc. Every time that the wire was brought into contact with the zinc, muscular contractions took place. I am well aware, that the advocates for metallic electricity will deduce from the plain statement of this fact, an induction contrary to Galvanism; but my candour, on this occasion, will show how much I am attached to the cause of truth.

EXPERIMENT IV.

Among animal bodies, the torpedo is one of those which produce the most powerful Galvanic action. In the autumn of 1801, I made some experiments on this animal at Genoa, in conjunction with Professor Mojon and his brother, who gave me every assistance in their power. When I touched the

the torpedo, under water, at the moment when it gave the shock, it contracted itself, and two jets of water proceeded from the two holes in its head. To obtain the shock, it was not necessary to touch two distinct parts of its body: in many cases, the application of the hand to the electric organ was sufficient.

PROPOSITION XI.

Mere electrization, by means of the common kinds of apparatus, does not increase the action of Galvanism.

EXPERIMENT I.

Artificial electricity was communicated to an apparatus, composed of a hundred cups, care being first taken to insulate the table and the persons who were afterwards to receive the action of it. If we suppose that the heterogeneous arcs were charged with different kinds of electricity, it would seem that, by communicating to them any electricity, the electricity of the whole apparatus ought to have been reduced to the same kind; consequently that no shock ought to have been produced. The contrary, however, was the case. We experienced very strong shocks, very little different from those which would have been obtained without artificial electricity. I observed the same result with the pile.

EXPERIMENT II.

An insulated torpedo being electrified, the shocks it gave were not increased. The torpedo was killed, and then armed, according to the method of Galvani, for the purpose of trying

ing whether metallic electricity, in this case, would have any influence over it. After this arrangement was made, every time that the conducting arc was applied to it strong contractions were produced; but very little different from those remarked in other animals. This observation is agreeable to those made at Naples by the celebrated Abilgaard, who, having subjected the torpedo to the Galvanic processes, found no extraordinary contractions.

PROPOSITION XII.

The Galvanic action is increased by employing as part of the arc the apparatus of Volta, or the electrified Leyden flask.

EXPERIMENT I.

In the hall of the Institute, I placed on a large table a hundred glass cups, and arranged them in such a manner as to form two rectangles, each composed of fifty. I established a communication between the first of these cups and the apparatus of Volta, by means of a metallic wire, which proceeded from one of the interior chambers of the *Cabinet de Physique*, and terminated at the place where the experiment was performed. I then tried this arrangement several times; and, however different opinions might be in regard to the precise increase of the action of the Galvanism, all constantly agreed in considering the shock as stronger. Some even went so far as to assert that it was increased a third.

It gave me great satisfaction to be able, on this occasion, to confirm the last discovery of Professor Volta, as well as one of those which he had made before. One observation, well attested, which tends to establish the truth of this proposition, is, that if a person touch the summit and base of the pile with two large metallic conductors, the shocks he receives will be much stronger.

EXPE-

EXPERIMENT II.

Electricity, concentrated in the Leyden flask, contributes also to increase the action of Galvanism. Having prepared a pile, composed of fifty plates of copper and zinc, I formed an arc by interposing a charged jar, and obtained an explosion much stronger than that obtained by the Leyden flask charged with an equal quantity of the electric fluid, and discharged independently of the pile.

EXPERIMENT III.

I took the same flask, after it was discharged, and having formed a portion of an arc, applied to the two extremities of the pile, I observed that the Galvanism refused to pass the obstacle presented to it by the stratum of glass interposed between the two coatings; consequently I received no shock.

EXPERIMENT IV.

I repeated the second experiment, insulating the pile, and at the same time the person who touched the pile with the charged flask. By these means I obtained a much stronger explosion than could have been produced separately by the Leyden flask or the pile. In this experiment I observed that the repeated passage of the electricity of the flask throughout the whole extent of the pile, did not deprive it of the property of exciting Galvanism.

PROPOSITION XIII.

Galvanism, in animals and in the pile, traverses large spaces with the same rapidity as the electric fluid.

EXPERIMENT.

I extended an iron wire, two hundred and fifty feet in length, around my chamber, taking great care that it should not any where touch itself, and made its extremities to terminate at a table which I had prepared for the experiment. One of these extremities being brought into communication with a pile composed of fifty plates of copper and zinc, I held the other in my left hand, and with my right touched the summit of the pile. The Galvanism then proceeded from the bottom of the pile to the summit, traversing a portion of the arc formed by the animal machine. By the effect of this passage, we may therefore form some opinion of the celerity of the Galvanic current. Its rapidity was such, that neither I nor any of those who repeated the experiment publicly, were able to determine the degree. The truth of this proposition is confirmed by experiments lately made by the celebrated Van Marum, who charged large batteries by means of Galvanism.

PRO-

PROPOSITION XIV.

The muscular contractions, which, according to the observations of Galvani, are produced by an electric atmosphere whether natural or artificial, correspond entirely with those produced by the pile, or by similar kinds of apparatus.

When a change of equilibrium takes place in those systems of bodies which communicate with the nerves and the muscles of the animal machine, it is always sensible of this change, and muscular contractions are produced.

EXPERIMENT I.

It is curious to see an animal, placed at the extremity of an apartment, experience a shock, when the electric spark is extracted at a considerable distance. I performed this experiment several times in the *Cabinet de Physique* of the Institute of Bologna, by means of a metal wire, not insulated, which was at the distance of four feet from the conductor of a common electrical machine. I repeated the experiment with crural nerves, having a ligature in the middle; and on extracting the spark, I observed violent contractions, which ceased when the ligature was formed at the

the place of their insertion into the muscles. I then performed with the new apparatus of Volta the experiments which Galvani had made with artificial electricity alone. At first I employed several glass cups and piles of from one to two hundred plates of metal; which proved to me that similar results might be obtained with the following simple apparatus.

EXPERIMENT II.

Having placed upon a table two glass vessels filled with salt water, (Plate II. fig. 5.) which I connected by means of an arc composed of brass and zinc, I applied to the surface of the water, in one of the vessels, the spinal marrow of a prepared frog, the corresponding muscles of which I held in one of my hands. Another person with his hand, or a plate of metal, then touched the water contained in the other vessel, and, at each contact, the muscles experienced violent contractions. To remove all idea of the contractions being produced by the action of the salt water, I connected with the spinal marrow a part of the muscles of another animal, which, instead of the spinal marrow, was made to touch the salt water, and obtained the same result.

EXPERIMENT III.

The same apparatus being retained, if either the person who touches the surface of the water in the first glass vessel, or the part of the frog immersed in the second, be insulated, no muscular contractions are produced; but they again take place when the insulation is removed. The violence of the contractions is increased by increasing the number of the glass vessels. If these vessels are made to communicate by means of arcs, formed of one homogeneous metal, the results are not different from those observed when heterogeneous metals are employed.

EXPERIMENT IV.

Being desirous to confirm the theory of the Galvanic atmosphere, I placed in it the body of an executed criminal. I removed the pile to the distance of a foot from the trunk, without the usual communication of metallic arcs; and having made an incision in each ancle, two persons held two frogs prepared in the usual manner, in such a position that the spinal marrow rested on the incisions. When matters were thus arranged, every time that a third person touched the summit of the pile, both the frogs experienced violent contractions, and to such a degree, that, leaving free one of the extremities, a real electrico-animal alarum was obtained, perfectly similar in its effect and identity to that described by Galvani in his Commentary. When the metallic apparatus

apparatus is employed, if one of the persons who holds a frog be insulated, the frog will remain motionless, while the other will experience the usual effect. I had an opportunity of confirming the truth of this observation, on the trunk of a dog, during a course of experiments made in the *Hôpital de la Charité*, at Paris, and at St. Thomas's Hospital, London.

PRO-

PROPOSITION XV.

Opium, cinchona, and other stimulants of a similar kind, which exercise a powerful action on the animal machine, contribute also to excite the action of the pile.

EXPERIMENT I.

In the last sitting of the Institute of Bologna, at which I was present, I constructed two piles, each composed of fifteen pieces of silver and zinc, employing for the one an extract of opium, and for the other an infusion of cinchona in alcohol. I covered the piles with two equal receivers; formed an insulated plenum, by pouring mercury around the bottoms of them; and placed weights on the receivers sufficient to prevent the mercury from raising them. At the end of some hours, I found a remarkable elevation of the surface of the mercury, so that the receivers remained fixed without requiring any weight to keep them down. In the bell which covered the pile where extract of opium had been employed, the mercury rose more than an inch; but in that covering the other, where cinchona had been used, the elevation of the mercury was scarcely three lines.

EXPERIMENT II.

Having successively introduced a taper into each of the two receivers before mentioned, it was immediately extinguished in that containing the pile with the extract of opium; but in the other containing the pile with the infusion of cinchona, it continued burning for some time. I found also that four of the plates, at the bottom of the pile where I employed opium, had suffered from the action of the mercury; and that some others, to the height of about three inches and a half, exhibited a few globules of that metal. The pile in which I employed infusion of cinchona showed scarcely any signs of the action of the mercury.

EXPERIMENT III.

As I concluded that the pile in which I had employed the alcoholic extract of opium possessed more activity than the other, I examined it for four or five days successively. I tried the flame of a taper with the same result as before, and assured myself that this pile retained the Galvanic power, though a little weakened, even till the eighth day. On the other hand, twenty-four hours had scarcely elapsed, when the pile in which alcohol and cinchona had been employed exhibited no signs of activity.

EXPERIMENT IV.

I constructed two piles, in the same manner, with pieces of pasteboard interposed, which had been previously moistened with strong solutions of camphor and of castor oil, in pure alcohol. In these two piles the Galvanic effects were much weaker in every respect than those observed in the preceding experiment; for, besides the explosion being less, the elevation of water in the insulated plenum was very small, and the alteration of the flame was scarcely sensible.

EXPERIMENT V.

I was able to convince myself that the effects of the two piles, before mentioned, were the immediate result of the substances dissolved, and not of the alcohol; for, having constructed a pile of thirty plates of silver and zinc, with pieces of pasteboard interposed, moistened with pure alcohol, I observed no signs of Galvanism; and the case was the same when I employed a pile of zinc combined with copper and other metals.

PROPOSITION XVI.

If the general relation between Galvanism and electricity be examined, such a correspondence will be found between them, as tends to confirm the analogy already stated.

To illustrate this proposition, I shall here take a view of the particular properties of electricity and Galvanism, which, if considered separately, would not be sufficient for my object. I am, however, of opinion, that when combined together they will serve to prove it in a satisfactory manner.

1st, Galvanism, like artificial electricity, emits sparks, fuses metals, and can even be employed to charge armed non-conducting bodies. I have proved the last-mentioned property, discovered by the celebrated Van Marum, with a new apparatus, composed of a pile with a hole in the middle, in which I place the flask I intend to charge.

2dly, The influence of artificial electricity tends to accelerate the putrefaction of animal parts; and the same phænomenon

nomenon may be produced by communication with the Voltaic pile, or by the first processes of Galvani.

3dly, The electricity of the Leyden jar is renewed in part immediately after its discharge. A similar phænomenon is exhibited by the pile; and it is observed, if the common Galvanic armatures be employed, that the vital force in animals is almost revived, when the arcs are applied different times.

4thly, As the action of common electricity and of the pile is suspended, when the combination of the metallic pieces is changed; in like manner, in a system of several animal machines, if their combination be changed, muscular contractions entirely cease.

5thly, Water may be decomposed by common electricity, as well as by the Galvanic pile, according to the ingenious method proposed by Dr. Wollaston. I lately saw, with the greatest pleasure, experiments on this subject performed by himself with the utmost neatness and precision. It is much to be wished, that the same result could be obtained by animal Galvanism alone; as it might tend to throw great light on some important points in physiology. For my part, I entertain no doubt that, after repeated trials, it may one day be effected, by means of large animals possessing a great abundance of animal electricity.

To

To conclude: I think I may venture to assert, that the correspondence between the properties of Galvanism and common electricity might be carried to a much greater extent, in confirmation of the analogy which I proposed to prove in this proposition.

PRO-

PROPOSITION XVII.

The hypothesis of an animal pile, analogous to that formed artificially, seems well calculated to explain the sensations and contractions in the animal machine.

It seems to be proved by the observations of Mr. Davy, Professor in the Royal Institution of Great Britain, and those of M. Gautherot at Paris, that a pile may be composed without any metallic substances whatever. We are therefore naturally led to suppose, that one may be composed also of animal substances alone. Though this has never yet been obtained by art, we behold it with admiration constructed by nature in various animals. If we examine, indeed, the structure of the regular bodies which succeed each other in the torpedo, the electrical eel of Surinam, and in the silurus, we shall find them to be real animal piles, differently arranged: and if an animal pile, exceedingly strong, be capable of communicating a shock, why should not one of a more moderate nature excite that activity which is necessary to produce muscular convulsions? I have already proved, that the system of the nerves and that of the muscles possess different Galvanic powers, or, as it were, different kinds of electricity, to which the animal moisture serves as a conductor. In this point of view, the discovery of the pile of the celebrated Volta, instead of destroying

destroying the principle of Galvanism, tends rather in a powerful manner to support it. The object of Galvani's system is to prove the existence of an animal electricity, and then to explain how its action operates in producing muscular sensations and contractions. The first part of his system rests upon facts, the truth of which neither time, nor the different experiments made by philosophers, have been able to weaken. The second presents an hypothesis which, perhaps, may be further illustrated when the physiology of the human body is better known. Galvani, to explain the activity of animal electricity, supposes the nerves and muscles to be like the Leyden jar; and this idea I confidently adopted. But by the expression '*Leyden flask*' he meant nothing else than that in the animal machine there are two opposite kinds of electricity, resulting from the nervous and the muscular systems, to which animal moisture continually serves as a vehicle. It was in this sense that he announced his theory of the Leyden flask, in his public lectures, and in his last works. No better comparison was then known, in the language of philosophy, to express this action. It however affords me great pleasure, that I can now substitute for it the pile discovered by Volta, which is perfectly consistent with the system of Galvani; and since I am ready to allow that the invention of the metallic pile gives Volta a title to the discovery of metallic electricity, I hope the discovery of animal electricity, properly so called, will be allowed to Galvani, as similar phænomena are exhibited by the nervous and muscular systems, independently of common electricity.

But

Fig. 5.
Fig. 4.
Fig. 6.
Fig. 3.
Fig. 1.
Fig. 2.

But some important questions in regard to Galvanism still remain to be answered, such as the following: whether the action of chemical combinations be the cause of Galvanism, or whether Galvanism be the cause of chemical combinations. In my opinion, we have not yet a sufficient number of data to determine this point. It may also be asked, whether Galvanism be of the same nature as electricity, but differently modified by the animal organization. For my part, until their identity be proved by further researches, I shall be contented with admitting that there is a great analogy between them.

But leaving these questions, the discussion of which might be premature, it will be better to deduce general corollaries from the series of experiments already detailed.

COROLLARY I.

It is found that there is a real attraction between certain parts of animals; and this tends to confirm the idea of a sort of atmosphere peculiar to parts of animals, as has been suggested by Humboldt. By these means it will perhaps be one day possible to explain, with less difficulty, the correspondence of some sensations in the animal machine.

COROLLARY II.

The action of Galvanism on the aëriform fluids may serve to explain its influence over the animal fluids by the oxida-

tion of the humours, and other phænomena which hitherto have been explained only in a hypothetical manner.

COROLLARY III.

Fishes, and several amphibious animals which live under water, sometimes approach the surface on certain changes of the atmosphere. When the before-mentioned experiments on the Galvanic atmosphere are considered, we may easily explain, why those changes which take place in distant parts of the atmosphere are communicated to the element in which these animals reside.

COROLLARY IV.

It has been ascertained, that water saturated with salts, and in particular with muriate of soda, contributes a great deal to increase the effects of Galvanism. It is well known also, that fishes, as compared with other animals, possess a very high degree of vitality; and hence we have reason to admire the wisdom of nature in making the sea, which is destined for the abode of fishes, to be abundantly saturated with muriate of soda.

COROLLARY V.

As Galvanism possesses great activity in chemical decompositions, it cannot remain in a state of inaction; but must necessarily

necessarily produce great changes in the animal fluids and functions.

COROLLARY VI.

This principle, to which some of the grand operations of nature have been entrusted, is not hypothetical; since it has been proved, that as there is a metallic arc and a metallic pile in the mineral kingdom, there is also an animal arc and an animal circle in the animal kingdom; which may one day throw great light on the progress of medicine, and be productive of considerable benefit to the human race.

PART THE SECOND.

ON THE INFLUENCE WHICH GALVANISM HAS ON THE VITAL POWERS.

To conduct an energetic fluid to the general seat of all impressions; to distribute its influence to the different parts of the nervous and muscular systems; to continue, revive, and, if I may be allowed the expression, to command the vital powers; such are the objects of my researches, and such the advantages which I purpose to derive from the action of Galvanism.

The discovery of the Galvanic pile by the celebrated Volta has served as a guide to enable me to obtain the most interesting results; and to these I have been conducted by numerous researches and a long series of experiments. I have examined the whole range of nature, and the grand family of animals has afforded me the means of making observations, highly interesting to physiology, on the whole œconomy of the vital powers. My experiments on this subject I shall divide into two Sections.

SECTION I.

Galvanism applied to various quadrupeds, birds, and other warm-blooded animals.

EXPERIMENT I.

The head of an ox, recently killed, was subjected to the action of a pile (Plate II. fig. 1.) composed of fifty plates of copper and zinc, separated, as usual, by small pieces of pasteboard moistened with a solution of muriate of soda. Having moistened one of the ears with the same solution, by means of a syringe, I introduced into it one extremity of a metallic wire. I then formed an arc with this wire to the summit of the pile, and by means of another wire made a communication between the bottom of the pile and the nostrils. When this apparatus was applied, the eyes were seen to open, the ears to shake, the tongue to be agitated, and the nostrils to swell, in the same manner as those of the living animal, when irritated and desirous of combating another of the same species.

I then moistened both the ears with salt water, by the same method as before, and inserted into each an extremity of one of the arcs. When the Galvanism was communicated, the

the movements already described were reproduced; but they appeared to be much more violent.

EXPERIMENT II.

A pile composed of a hundred pieces of silver and zinc (Plate II. fig. 2.) being employed, the tongue issued from the mouth four inches, and re-entered it an inch, on each application of the arc; notwithstanding the resistance opposed by the teeth which pressed against it: so that after four or five applications of the arc it was entirely restored to its usual situation.

I repeated this curious experiment several times at Bologna and Turin, and lately at London before their Royal Highnesses the Prince of Wales, the Duke of York, the Duke of Clarence, and the Duke of Cumberland, who seemed to be much interested in my researches. I showed them that the tongue returned without being touched, merely by forming an arc between distant parts, such as the spinal marrow and the cervical or nasal muscles. A person who held the extremity of the tongue with a pair of pincers felt the effort it made to return every time that the Galvanism was applied.

EXPERIMENT III.

With the same apparatus I suspended from the extremity of the conducting arc the posterior half of a frog,

by

by bending the iron wire at right angles into a small elbow; and then, instead of making the tongue touch the extremity of the arc, I brought it into contact with one of the paws of the frog, while the other extremity of the wire rested on the summit of the pile.

When this arrangement was made, I not only obtained the same contractions in the head of the ox, but I observed also that when the paw of the frog ceased to be in contact with the tongue, it was attracted by the latter, which produced in it oscillations, so that it formed a kind of Galvanometer; for the thighs of the small animal diverged more or less according to the intensity of the fluid which passed through them, and were restored to their former position when the paw of the frog and the tongue of the ox were again brought into contact. These oscillations continued about six minutes.

Suspecting, however, that the crural nerves might have some share in these phænomena, independently of the pile, I cut these nerves, and under similar conditions I obtained the same results.

EXPERIMENT IV.

Being desirous to repeat the above experiments on the heads of other oxen, and on those of sheep and lambs, varying the pile, both in regard to its nature and the number of pieces, I constructed three piles of twenty-five, fifty, and

a hundred

a hundred and twenty pieces of silver and zinc. The results, however, differed from the preceding only in the greater or less intensity of the contractions, according as one or the other apparatus was applied to the same animals. I remarked in particular, that the combination most favourable to muscular contractions is obtained, when the arc is established from the ears to the spinal marrow. In this case the eye is so much affected, that the eye-lids open entirely while the eye-ball turns round, and projects somewhat from its socket, as sometimes happens in the most violent madness.

EXPERIMENT V.

Having provided an ox recently killed, the head of which was not cut off, I formed an arc from one ear to the other, interposing the pile. The immediate result was a commotion so violent in all the extremities of the animal, that several of the spectators were much alarmed, and thought it prudent to retire to some distance. I then cut off the head, and formed an arc from the spinal marrow, first to the diaphragm, and then to the sphincter ani. In the first case, the diaphragm experienced violent contractions; in the other I obtained a very strong action on the rectum, which even produced an expulsion of the fæces.

EXPERIMENT VI.

To give more extent to my experiments, I thought proper

to repeat them on lambs, chickens, and other warm-blooded animals; and without enumerating such phænomena as are common, I shall only observe, that the tongue, which was projected beyond the lips, again returned into the cavity of the mouth, after several applications of the arc, as was the case in the second experiment. The movements of the ears and eye-lids were stronger than in the other parts. Comparative anatomy must explain why this phænomenon, so striking in animals of this kind, is not observed in man.

EXPERIMENT VII.

The observations which I had made on the Galvanism of the pile excited my curiosity so much, that I was induced to try some comparative experiments by means of common electricity. With this view I placed an iron wire in each ear of a lamb, and discharged through it twice in succession a Leyden flask, the two coatings of which were in communication with the wires applied to the ears. By these means I obtained contractions, but weaker than those produced by the pile; and I always observed the same result in other warm-blooded animals.

EXPERIMENT VIII.

Having repeated the same experiments on live chickens, I found, to my great surprise, notwithstanding the weakness of their organization, that they sustained with firmness the

strongest

strongest shocks, communicated several times, with a pile composed of fifty plates of silver and zinc. Though apparently dejected, and almost on the point of expiring,—as soon as I interrupted the action of the pile, they fluttered their wings, and seemed to congratulate themselves on their escape from danger.

The curiosity natural to a philosopher induced me to subject these birds to anatomical dissection, that I might examine what effects had been produced on the animal machine by these convulsions. The principal phænomena which I observed were extravasated blood in the muscles; a derangement of the humours in different parts; the intestines removed from their usual seat, and thrown towards the pelvis. At some future period, I purpose to examine how long these animals are capable of living under the continued action of Galvanism, applied to them in different ways.

EXPERIMENT IX.

I applied the Galvanic action to a pullet just killed, forming an arc from one of the ears to the other. When this arrangement was made, I observed contractions not only in the feet, but also in the wings and the whole animal machine. The same phænomena were produced by the same means in two other pullets. I then combined the different parts of these three pullets in such a manner, that the head of the second was joined to the foot of the first, and

the head of the third to the head of the second. An arc being then formed to the two extremities of this chain of animal parts, I was much pleased to see the three pullets move their wings and their feet at the same time.

EXPERIMENT X.

The results of the preceding experiments led me to examine the power of an arc formed by animal moisture. For this purpose, having connected the heads of two oxen, (Plate II. fig. 3.) by bringing near each other the sections of the neck, I established an arc from the summit of the pile to one of the ears of one head, and another from the base of the pile to one of the ears of the other. When this arrangement was made, I observed that both the heads exhibited evident signs of muscular contractions.

EXPERIMENT XI.

The trunks of two calves being united by the sections of the neck, and an arc being established by the interposition of the pile from the anus of the one to that of the other, both the trunks received, at the same time, a commotion; but not very violent. I repeated this experiment on the trunks of two lambs, but with a more striking result, as all the extremities and muscles experienced violent convulsions. A glass vessel, employed for the experiments, which stood on the table,

table, was overturned by one of the extremities, and thrown to the distance of about two feet. I tried other combinations, but the contractions were weaker.

EXPERIMENT XII.

Having sawn open the skull, I directed the action of the pile to different parts of the brain, in the same order as they occurred in the course of anatomical dissection. All these parts appeared to be affected by the Galvanic force; but its action was stronger on the corpus callosum and the cerebellum. The same result nearly was obtained, when I repeated the experiment on the heads of different calves and lambs.

EXPERIMENT XIII.

The heart of an ox, removed from the body, being exposed to the action of Galvanism, though the pile was very powerful, exhibited no signs of muscular contraction. I repeated the same experiment on the heart of an ox, without removing it from the body, and on the hearts of several dogs, one arc being applied to the spinal marrow, while the other touched sometimes the surface of the heart, and sometimes penetrated into its substance; but with the same result: no muscular convulsions were produced.

EXPERIMENT XIV.

I prepared some frogs; and having waited till the motion of the ventricles of the heart had become very slow, and almost imperceptible, I communicated to them the Galvanic influence, and it appeared to me that some movements were produced in the ventricles. I repeated this experiment lately on the heart of a rabbit, and with the same success. Having tried the hearts of several calves and dogs, I could not observe any decided motion in the ventricles; but I remarked that the Galvanic power exercised a strong action on the auricles.

EXPERIMENT XV.

Without taking into consideration the differences in the action of Galvanism on the heart, according to the different applications and the different kinds of animals subjected to experiment, I observed, that after this muscle has lost its susceptibility to the action of Galvanism, the other muscles still retain it in a very high degree. This effect is very striking in regard to the heart and the muscles of oxen and dogs; and this corresponds with what has been stated by the Commissioners of the French National Institute in their Report. Speaking of the anomalies found in this respect in the heart, they conclude *that it is at any rate certain that this organ loses, in a very short time, and much sooner than the other muscles, the faculty of being agitated by Galvanism.*

EXPE-

EXPERIMENT XVI.

All the observations I was able to make on the involuntary muscles will be found in the same Report, from which the following is an extract: "Dr. Grapengiesser says, that he saw the vermicular motion of the intestines increased by the action of Galvanism in a living subject, whose large intestines protruded beyond the abdomen, in consequence of a scrotal hernia. Professor Aldini made us observe the same effect on the intestinal canal of a dog. We perceived also very evident contractions in a portion of the stomach, separated from the animal. We saw the auricles of the heart contract; but never found this to be the case with the ventricles."

EXPERIMENT XVII.

As I found it difficult, in the course of my travels, to obtain large animals for my experiments, a desire of prosecuting my researches induced me to be satisfied with such small animals as were easiest to be procured. I therefore declared war against the dogs, which exhibited the same phænomena as oxen, and with the greatest energy, as may be seen by the following extract from the before-mentioned Report of the Commissioners of the French National Institute: "The head of a dog being cut off, Aldini subjected it to the action of a strong pile, by which means the most frightful convulsions were produced. The mouth opened, the teeth gnashed, the

eyes

eyes rolled in their orbits; and, if the imagination had not been restrained by reason and reflection, one might have almost believed that the animal was restored to life, and in a state of agony."

EXPERIMENT XVIII.

The head and trunk of a dog, separated from each other, and placed in such a manner as to leave an interval of about a foot between them (Plate II. fig. 4.), were made to move simultaneously by applying the Galvanic action to one of the ears, and to a small incision made in one of the extremities of the trunk. I saw the same effect produced in a public sitting held at the *Hôpital de la Charité* at Paris. In this case, the distance between the head and trunk was a foot and a half.

EXPERIMENT XIX.

In the preceding experiments, it is always necessary that the part of the table which forms the interval between the head and trunk should be moistened with salt water, or some other *conducting fluid*. Considered in this point of view, the head and trunk mutually form an arc which conducts the Galvanic action; so that the contractions excited at the same time do not depend on the particular organization of the animals subjected to experiment. This I confirmed by producing simultaneous contractions in the trunk of a dog combined with the head of a rabbit, and *vice versa*.

EXPE-

EXPERIMENT XX.

At the School of Medicine at Paris, in presence of the Commissioners of the French National Institute, and of Professor Huzzard, I tried the action of Galvanism on a horse which had been killed by the insufflation of air into the jugular veins. The trunk exhibited no extraordinary motion; but the head was violently agitated. A very sensible gnashing of the teeth was produced, and all the muscles performed, in a surprising manner, the same motion as is exerted during the time of mastication. There was even a visible excretion of the saliva. Of all the heads hitherto tried, that of the horse exhibited the most violent motion by the action of Galvanism.

EXPERIMENT XXI.

Having performed this series of experiments, it was necessary that a comparison should be made, cæteris paribus, between the action of those stimulants proposed by the celebrated Haller, and the means here used to excite the action of Galvanism. For this purpose I employed a head weakened to such a degree that it was no longer sensible to the action of the Hallerian stimulants applied to the muscles and nerves, and then to different parts of the brain

brain laid bare, and separated one from the other. I tried the action of the sulphuric and nitric acids, and the effect of the bistouri, but without ever producing the smallest contraction in warm-blooded animals: on the other hand, the action of Galvanism, on these parts, in the above state, occasioned very powerful muscular contractions.

SECTION II.

Experiments made on human bodies after death.

From the experiments already described, one might by analogy conjecture what effect the action of Galvanism would produce on that noble being man, the sole object of my researches. But to enable philosophers to judge with more certainty respecting the effects of this wonderful agent, it was necessary to adhere to certain conditions, and to apply it immediately after death. The bodies of persons who had died of disease were not proper for my purpose; because it is to be presumed, that the development of the principle which occasions death destroys the elasticity of the fibres, and that the humours are changed from their natural to a corrupted state. It was therefore necessary to obtain the human body while it still retained, after death, the vital powers in the highest degree of preservation; and hence I was obliged, if I may be allowed the expression, to place myself under the scaffold, near the axe of justice, to receive the yet bleeding bodies of unfortunate criminals, the only subjects proper for my experiments. In consequence of an application made for that purpose, I obtained from Government the bodies of two brigands, who were decapitated at Bologna in the month of January 1802. As both these in-

dividuals had been very young, and of a robust constitution, and as the parts exhibited the utmost soundness, I entertained strong hopes of obtaining the happiest results from my proposed researches. Though accustomed to a more tranquil kind of operations in my closet, and little acquainted with anatomical dissections, the love of truth, and a desire to throw some light on the system of Galvanism, overcame all my repugnance, and I proceeded to the following experiments.

EXPERIMENT XXII.

The first of these decapitated criminals being conveyed to the apartment provided for my experiments, in the neighbourhood of the place of execution, the head was first subjected to the Galvanic action. For this purpose I had constructed a pile consisting of a hundred pieces of silver and zinc. Having moistened the inside of the ears with salt water, I formed an arc with two metallic wires, which, proceeding from the two ears, were applied, one to the summit and the other to the bottom of the pile. When this communication was established, I observed strong contractions in all the muscles of the face, which were contorted in so irregular a manner that they exhibited the appearance of the most horrid grimaces. The action of the eye-lids was exceedingly striking, though less sensible in the human head than in that of the ox.

EXPERIMENT XXIII.

Having established an arc from the top of the left ear, and then from the bottom of that ear to the tongue, drawn about an inch without the mouth, contractions were observed in the face, and the tongue sensibly returned into the mouth. I then touched the upper or lower lips, and obtained contractions, which were remarkable chiefly in all the muscles of the left part of the face; so that the mouth appeared as if distorted by a partial kind of palsy. On the first application of the arc, a small quantity of saliva was discharged from the mouth.

EXPERIMENT XXIV.

I caused the head to be shaved exactly above the parietal protuberance on the right side; and having moistened the integuments, armed with silver and zinc, I established a communication by means of the pile between the parietal bone and one of the ears. I obtained contractions, but weaker than those observed when the arcs were formed according to the different methods already described.

EXPERIMENT XXV.

Having formed an arc from the ears to different parts of the

the face, moistened with a solution of muriate of soda, such for example as the nose and forehead, I always observed violent contractions. But the contractions were stronger when, instead of the first-mentioned pile, I employed another consisting of fifty plates of copper and zinc. I even still decreased the number of plates, in order that I might try, in the course of these experiments, the different degrees of activity which the pile would exhibit.

EXPERIMENT XXVI.

The head of the other criminal being brought to me after I had employed about half an hour in these experiments, I repeated them on this second head, and found the results to be analogous to those before obtained. But the contractions produced in the second head were stronger in consequence of its greater vitality: the vitality of the first seemed to have been nearly exhausted.

EXPERIMENT XXVII.

Being desirous to examine, according to the principles of Galvani, the power of an arc of animal moisture in warm-blooded animals, I recollected that I had several times observed simultaneous convulsions produced by these means in two frogs, and recently in the heads of two oxen, the arc being conveyed from the one to the other in different ways.

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I placed the two heads in a straight line on a table, in such a manner that the sections of the neck were brought into communication merely by the animal fluids. When thus arranged, I formed an arc from the pile to the right ear of one head, and to the left ear of the other, and saw with astonishment the two heads make horrid grimaces; so that the spectators, who had no suspicion of such a result, were actually frightened. It was however observed, that the convulsions excited in the heads disposed in this manner, were not so strong as those produced when I performed the experiment on each head separately. It is certain that, in this experiment, the arc of animal moisture supplies the place of a continuation of the nervous and muscular fibres.

EXPERIMENT XXVIII.

Having tried the effect of Galvanism on the exterior part of the head, I proceeded to examine the phænomena exhibited by the interior organs when treated in the same manner. I therefore removed the upper part of the cranium by a section parallel to its base, uncovered the pia mater, and established an arc from one of the ears to the medullary substance. On the application of the arc strong convulsions were observed in the face. While preparing the brain for my experiments, I remarked that, in dividing the muscles of the forehead, at each stroke of the dissecting knife, very strong contractions, which continued after the dissection

was

was finished, were excited in the muscles of the face. I was informed that this is an uncommon phænomenon in anatomical dissections; and therefore I shall leave it to anatomists to determine whether it was occasioned, either in whole or in part, by the preceding action of the pile.

EXPERIMENT XXIX.

Having then separated the lobes of the brain, I applied the arc to the corpus callosum, to the ears or to the lips, and found that the whole osseous box and the muscles of the face were violently agitated. Some of the spectators even imagined that the corpus callosum itself was affected by a peculiar convulsion; but it is possible that this emotion was owing to a mechanical impulse which shook the whole head. New experiments will, therefore, be necessary before any thing further can be said in regard to this observation.

EXPERIMENT XXX.

Having carried the dissection to the olfactory nerves, and even to the crossing of the optic nerves, I formed an arc from these parts to the lips and the eyes, and obtained contractions, but very weak in comparison of the preceding. I observed that on touching the optic nerves with one of the arcs no sensible convulsions were produced in the eye-lids.

EXPERIMENT XXXI.

This mutilated head, which had been so long the subject of observation, was united by the plane of the section to that of the other criminal, which had not been subjected to anatomical dissection. I then applied two arcs, making one of them to communicate with the summit of the pile and the right ear of one head, and the other with the bottom of the pile and the left ear of the second head. Both heads experienced contractions similar to those described in the 27th experiment; but in the head which had already been employed they appeared to be weaker.

EXPERIMENT XXXII.

After these experiments on the head, I proceeded to the trunk of the second criminal, which I conceived to be most proper for my purpose.

I think it necessary here to observe, that the body had been exposed for about an hour, in an open court, where the temperature was two degrees below zero. The muscles of the fore-arm and the tendinous parts of the metacarpus being laid bare, an arc was established from those muscles to the spinal marrow. In consequence of this arrangement, the fore-arm was raised, to the great astonishment of those who were present.

EXPERIMENT XXXIII.

Having established an arc between the biceps muscle of each arm, which I had laid perfectly bare, I obtained similar contractions, but somewhat weaker than in the preceding case.

EXPERIMENT XXXIV.

Having laid bare the tendons of the fingers, on the back of the hand, I established an arc between that region and the spinal marrow, and obtained strong contractions in the fingers and in the whole hand.

EXPERIMENT XXXV.

Proceeding to the lower extremities, I formed an arc from the spinal marrow to the vastus internus, vastus externus, sartorius, and other muscles, and obtained strong contractions in all these muscles. Having removed the arcs and the pile, the muscles retained a small oscillatory motion, which continued for ten minutes. I observed the same phænomenon in the muscles of the neck, when I established an arc between the spinal marrow and various other parts of the trunk.

EXPERIMENT XXXVI.

Having applied the arc to the spinal marrow and the uncovered muscles of the under part of the tarsus of the right foot, the extensor muscles of all the toes, and particularly of the great toe, experienced very sensible contractions. I repeated the experiment with the arc applied, not to the spinal marrow, but to the uncovered muscles of the thigh, employed in the preceding experiment, and found the contractions excited to be much stronger. In like manner, the muscles of the soles of the feet, when I established an arc between them and the muscles of the thigh, manifested much stronger contractions than when the arc extended to any other distant part.

EXPERIMENT XXXVII.

Having examined the force of the contractions, when the arcs were applied to the surface of the muscles of the extremities, I tried what effect would be produced by introducing them into their substance. In this case, the energy of the contractions was much increased.

EXPERIMENT XXXVIII.

After trying the action of Galvanism on the extremities,

I resolved to examine the trunk. With this view, having established an arc from the spinal marrow to the muscles of the diaphragm, I obtained very sensible contractions every time the arc was applied.

EXPERIMENT XXXIX.

I then caused the thorax to be opened, that I might try the effects of Galvanism on the most important of all the muscles, the heart. The pericardium having been detached, I applied the conductor to the principal organ of life, and I even caused it to be opened, to examine whether there existed in any of its folds some fibre susceptible of oscillation; but my researches were fruitless. This insensibility ought, perhaps, to be ascribed to the want of a certain degree of heat and of animal moisture, not to be found in a body two hours after death. It will, therefore, be proper to repeat this experiment, taking care to observe all those conditions which may be necessary to ensure its success.

EXPERIMENT XL.

In the preceding experiment I observed that the diaphragm contracted, and that the blood, which after this phænomenon I supposed to be coagulated, flowed on the contrary from the vena cava inferior, and the jugular veins, the moment the arc was applied, and appeared of a bright red colour.

colour. Is there reason to conjecture that, though great contractions cannot be produced, it is possible to excite in the interior parts of the heart some oscillations analogous to those which I observed in the muscles of the thigh and neck? This question can be determined only by new experiments.

EXPERIMENT XLI.

I observed in these experiments, that the more the points of contact of the arc with the biceps muscle were multiplied, the more the motion of the arm was extended; especially when care was taken to insulate the muscle by removing the integuments, and surrounding it with the wire bent in the form of a ring. Having applied arcs to the biceps muscle of each arm, I was much surprised to see the fore-arm and hand of the extremity, where the before-mentioned ring was placed, rise quickly to the height of about six inches.

EXPERIMENT XLII.

I repeated the experiment, forming the arc from the biceps muscle of the fore-arm to the spinal marrow. By these means contractions so violent were excited, that the anterior part of the arm, the whole of which lay extended in a horizontal position, rose seven inches above the plane of the table. Having placed on the palm of the hand a metallic body, such as a piece of money, the hand at first supported

ported it for a little time; but at a certain degree of elevation it projected it to some distance. I then substituted for the piece of money a pair of iron pincers, about half a pound in weight; the hand rose up and seemed to seize them; but at the highest degree of elevation the contraction ceased, and the pincers fell. I observed that the weight with which the hand was loaded, diminished the elevating power of the arm very little. It may be proper to remark, that the last two experiments were performed an hour and a quarter after the execution, and those on the lower extremities almost two hours.

If this experiment were speedily repeated, in order to take advantage of the highest degree of vitality, loading the hand with different weights in succession, till the motion of the hand should be totally impeded, I am of opinion that an estimate might be formed of the elevating force, according to the different degrees of vitality.

In the preceding experiments I have omitted certain observations, which did not agree with those made on other warm-blooded animals. But my silence deranges no theory; and, besides, facts not sufficiently confirmed would have led me into physiological discussions of little utility, as these points can be determined only by new experiments.

It is painful to a philosopher to reflect, that his doubts cannot be cleared up until new victims shall fall under the sword of justice; but the hope that his researches may lead

to

to some new discovery beneficial to mankind, in a physiological point of view, lessens in a certain measure the disagreeable sensations excited by these melancholy scenes.

An assassin decapitated at Bologna, in the month of January 1802, afforded me an opportunity of verifying the truth of my former observations. In my first experiments on the human body, the dura and pia mater had been so little affected, that several of those present concluded that these parts were absolutely insensible to the action of the Galvanic pile; and this opinion was still further strengthened by the authority of several able anatomists. On the other hand, many warm-blooded animals, as will be seen hereafter, afforded contrary results. It is certain that nature, which is always consistent with itself in the action of that general principle which excites the muscular forces, ought to exhibit in the human body the same phænomena as those which constantly occur in other warm-blooded animals. I mentioned this anomaly to several of my colleagues, and particularly to that able anatomist Mondini, who, having made researches on this subject, communicated to me the result of them, and directed all the preparations necessary for the following experiments, modified according to the general views collected in the preceding observations.

EXPERIMENT XLIII.

The body of the criminal having been removed from the place

place of execution to a neighbouring apartment, the trunk was placed on one table, and the head on another. On the latter stood a pile, composed of a hundred plates of zinc and copper, and on the former, a pile of a hundred plates of silver and zinc. This apparatus tended greatly to facilitate the rapid performance of the experiments, which were made at the same time on all the parts of the body, and enabled us to take advantage of its great vitality. I had with me a select company of young physicians and surgeons, who, being much interested in the progress of Galvanism, assisted me with great ardour. They were divided into two parties, each of which was stationed around one of the tables, in order that the operations performed at the one might not interrupt those at the other. To gratify Professor Mondini, who was desirous of seeing the muscular action in the whole head, an arc was established from the spinal marrow to one of the ears, the pile being interposed between them, and strong contractions were produced in the whole face, as had been the case in all the other animals.

EXPERIMENT XLIV.

Having sawn through the scull with every possible precaution, and an arc being established from the dura mater to one of the ears, the usual contractions ensued. The pia mater was then uncovered, and, by employing the same means, the same results and the same effects were produced as had been obtained in other animals.

EXPE-

EXPERIMENT XLV.

The cortical substance in the left hemisphere was uncovered, and an arc being extended from it to the right ear, the movements of the face were exceedingly sensible in the part opposite to the uncovered hemisphere. I repeated the experiment different ways with the same results, and found, as I suspected, that this process put an end to the anomaly observed in regard to the membranes and cortical substance, which at first seemed to withstand the general action of the Galvanic fluid, though it produced an effect on the other parts of the brain. Dr. Mondini took care to remove the superfluous moisture, both from the membranes and cortical substance, in order to prevent all suspicion that it might facilitate a communication, in any manner, with the medullary substance.

EXPERIMENT XLVI.

Dr. Mondini, with his usual ability, having exposed in the brain the medullary substance, the corpora striata, the corpus callosum, the thalami nervorum opticorum, and the cerebellum; and an arc being formed of all these parts, we fully confirmed the results of the experiments which had been before made on the bodies of other criminals.

EXPERIMENT XLVII.

Having observed these phænomena in the head, we proceeded to the trunk, which gave us results no less interesting. The body was sound and robust, and indicated a constitution replete with vital energy. By forming an arc from the spinal marrow to the biceps muscle, very strong movements were produced throughout the whole body, and particularly in the arm, which could not be bent without very great exertion.

EXPERIMENT XLVIII.

By again applying the arc, according to the method detailed in the 41st experiment, the violence of the contractions was much increased. The trunk was thrown into strong convulsions; the shoulders were elevated in a sensible manner; and the hands were so agitated that they beat against the table which supported the body.

EXPERIMENT XLIX.

A silver probe was inserted into the spinal marrow, and one of the hands immersed in a solution of muriate of soda. I then applied one extremity of the arc to the most distant part of the probe, and the other to the surface of the water, by which means the Galvanism was made to exercise its ac-

tion

tion without any immediate contact of the animal parts. When this arrangement was formed, the arm, which hung over the edge of the table, moved towards the breast, passing over the space of about a foot and a half. By employing, at the same time, the two piles composed of a hundred plates of copper and zinc, the contractions were much increased. But this augmentation of force did not exactly follow the ratio of the combined activity of the two piles united.

EXPERIMENT L.

By forming an arc from the feet to the spinal marrow, first applying armatures, and then employing a solution of muriate of soda as in the preceding experiments, I obtained contractions, but weaker than those in the upper extremities. As there was reason to suspect that this diminution arose in part from the position of the trunk, I placed the body in such a manner, that, while the thighs rested on the edge of the table, the legs, which hung over it, were at full liberty to move. This difference in position produced a difference in the results; and I intend to repeat the experiment, according to this arrangement, applying the arc directly to the crural nerves.

EXPERIMENT LI.

A small portion of the great pectoral muscle being de-

tached from the ribs, and exposed to the action of the pile, exhibited strong contractions, and the motion of the diaphragm was also very remarkable. The heart alone, though carefully subjected to all the operations detailed in the 47th experiment, remained perfectly motionless.

EXPERIMENT LII.

Having brought the separated head near to the neck of the trunk, I established a communication between it and the trunk by means of the animal moisture alone; and an arc being then formed from the head to different parts of the trunk, sensible contractions were produced, and particularly in the latter. This observation seems still further to prove that an arc of moisture has power to excite muscular contractions. In this, as well as in the preceding experiments, if any of the spectators, while an arc was established by means of the pile, brought a frog prepared in the usual manner near to the human body, the frog experienced strong contractions, though at a distance from the place to which the action of the Galvanism was determined.

EXPERIMENT LIII.

After three hours had been employed in these experiments, I conceived the idea of trying the action of Galvanism on some parts of the body separated from the trunk. One

One of the legs, therefore, was amputated six inches above the joint of the knee; and an arc being established from a point in the plane of the section to the knee, I obtained contractions similar to those which had been produced before the limb was separated from the body. I then formed an arc of moisture, by applying the amputated limb to the trunk; and having determined the action of the pile to the spinal marrow and the foot, I obtained very sensible contractions. I observed, on this occasion, that a frog, prepared some time before, and which was accidentally lying on the table, at each application of the metallic arcs moved like an electrometer, and thus confirmed the action of an arc of moisture.

EXPERIMENT LIV.

After a considerable time had been employed in the preceding experiment, I endeavoured to revive the action of the Galvanism by moistening the muscles with a solution of opium. By these means the contractions seemed to be increased; and the case was the same in other warm-blooded animals. A series of similar experiments, if carefully made, would no doubt be attended with important results; as they might enable us to ascertain the action of the different stimulants proposed by Dr. Brown. But I must, in the mean time, observe, that the before-mentioned effects of opium fully correspond with those long ago observed by Galvani.

It results, in general, from my experiments, that moisture performs a conspicuous part in producing contractions; and that it is even of more importance than animal heat. I indeed find that muscular contractions may be obtained after the body has thrown out a great deal of its heat, even when it has cooled for several hours, and when it has been exposed to a temperature below zero; for, if Galvanism be communicated to a body in that state, muscular contractions will be immediately excited; but they soon cease by the privation of animal moisture. If a muscle, indeed, which has been laid bare resists the Galvanic influence, its action may be speedily renewed by making an incision into it, or into some of the muscles which surround it. I can assert, that by this process partial contractions were produced in the human body five hours after death, every time that the arcs were applied to the muscular fibres.

Being worn out with this long series of experiments, I found it necessary to abandon them; but, from the force of the contractions, it could easily be seen that they might have been produced much longer.

Having communicated these results to the celebrated Caldani, Professor of Anatomy in the University of Padua, he requested that I would confirm the observations I had made by again applying Galvanism to the membranes and to the cortical substance of the brain. He was unwilling to give up the system of Haller without very positive proofs; and his doubts were to me of great utility, as they induced me to establish

establish the action of Galvanism on these parts by the following experiments:

EXPERIMENT LV.

As these doubts related to a delicate point, which would have produced some variation in the theory of a celebrated physiologist, I repeated the experiment on the head of an ox newly killed, in the presence of Professor Mondini, who made the necessary preparations. The dura mater was laid bare; and the action of the Galvanism being conducted to it, strong contractions were immediately produced. The same phænomenon took place when the cortical substance was brought into contact with one of the arcs. I repeated this experiment with the same success on the heads of several oxen and lambs.

EXPERIMENT LVI.

When I passed through Turin, Professors Vassalli, Giulio, and Rossi requested me to perform, in their presence, my principal experiments, and those in particular which related to the membranes and the cortical substance of the brain. They observed, at the same time, that in uncovering the brain of an ox with a cleaver, some derangement, in consequence of the agitation, might be effected in that organ, which would perhaps produce an alteration in the results. Professors Giulio and Rossi proposed therefore to uncover the brain by the trepan, which gave a greater degree of precision to the

the experiment. The dura mater of an ox being uncovered in this manner, it was subjected to the action of Galvanism, and even with this mode of preparation the muscular contractions every time the arc was applied were pretty strong. The arc being determined to the cortical substance, the force of the contractions seemed to be increased; and in general they appeared to be more considerable in proportion as the arc was plunged to a greater depth into the substance of the brain.

These gentlemen, in whose presence I performed my principal experiments, having pursued my method after my departure, made some observations which may be of great use to physiology. They had before entertained strong doubts whether the heart was susceptible of the Galvanic action, by means of simple armatures applied to the different parts of that muscle; and such irregularity had been observed in the results, that it appeared difficult to determine the question. I was therefore exceedingly happy that I had had an opportunity of exciting in others a desire to make new researches in regard to this point, of so much importance. Professors Vassalli, Giulio and Rossi repeated and modified my experiments on the heart in such a manner, that they obtained in man, and warm-blooded animals, contractions similar to those which I had observed only in cold-blooded animals. I wait for a detailed account of these experiments, as well as of others made on decapitated criminals*, which

* Some account of the latter, from the *Journal de Physique,* will be found in the Appendix.—Trans.

were

were subjected to my processes. The bodies were removed to the large hospital; and notwithstanding the long period which had elapsed between the time of the execution and that when the experiments were performed, they observed almost the same phænomena.

The observation made on this criminal, that the vital powers exist so long in the body after death, induces me to hope that I shall be able to obtain similar results by the application of my method, in common cases of suspended animation. But when employed for this laudable purpose it should be modified in such a manner as to render unnecessary those operations which are so painful to humanity. On this account, I have invented a method by which muscular contractions may be produced without the least incision or separation of the muscles. It is so combined, that it cannot be rejected by the most rigorous medical jurisprudence.

EXPERIMENT LVII.

In order to try the vital force existing in the human body after death, I immerse the hand in a solution of muriate of soda, and establish an arc, one of the extremities of which is made to pass round the fore-arm, while the other is brought into contact with the bottom of the pile. I adapt to the extremity of another arc an elastic probe, which is applied to one of the ears, moistened by means of a sy-

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ringe with the same solution, and connect the other extremity of the arc with the summit of the pile. By this arrangement various contractions, according to the different degrees of vitality in the bodies, are observed, sometimes in the fingers, sometimes in the hand, and sometimes in the whole arm. The fingers bend, and move in a sensible manner; and sometimes the whole of the fore-arm proceeds towards the breast. The importance of this method for determining the duration of the vital powers after death may be readily comprehended. Should means be found hereafter to make further discoveries in regard to this interesting point, physiologists may then be able to determine with certainty those cases when interment ought to be retarded; and those where the good of society requires that every possible means of resuscitation should be employed. In the large hospital of Bologna, I made several observations on this subject; and remarked in particular, how much the nature of the disease contributes, *cæteris paribus*, to produce a difference in the duration of the muscular contractions. I tried the case of death produced by putrid fevers, by pleurisies, by wounds in the pericardium, by the scurvy, and by the consequences of parturition; and I found a great difference in the degrees of vitality, according to the circumstances of the disease, the age and temperament of the individual; which confirms me in the opinion I entertain, that these experiments, if long continued without intermission by able physiologists, might be of the greatest benefit to medicine.

These

These researches are not an object of mere curiosity; they seem to open an extensive field for promoting the welfare of the human race, and may be of service in cases of apparent death, occasioned by an alteration of the brain, and sometimes in cases of asphyxia. Various learned academies are entitled to great praise for having turned their attention to this subject, and for having already recommended different stimulants as proper for being used on such occasions. But I must take the liberty of requesting that in similar cases the action of Galvanism may be tried, by employing the new method here proposed. It is of great importance that the means of affording relief to the sufferings of mankind should be multiplied, and especially in cases in which the old system of medicine presents to us so few resources. In the mean time I conceive it may be useful to make some trials on animals thrown into a state of asphyxia different ways. These researches may lead to valuable discoveries, and produce some light to direct us in our attempts to save the lives of men. If the encouragement I have received from the medical and philosophical world, in general, induce others to pursue the same path, it will give me great satisfaction. Galvanism is yet in its infancy; and when we reflect on the slow progress which many other branches of science have made, and how long they remained almost stationary before the full importance of them was known, it would be presumption to set bounds to that which is the subject of the present work. For my part, I spared no pains during my short stay at Paris to exhibit my method, and

to make it publicly known. Dr. Pinel assisted at my experiments with the utmost zeal, and was witness to muscular contractions excited in the body of an old woman, who had died of a malignant putrid fever. The interest which he took in my researches induced me to communicate to him several plans I had formed for giving relief to some of those unfortunate beings committed to his skill and beneficent care in the *Hôpital des Foux.* I pointed out to him some particular cases where the individuals, in consequence of a deep rooted melancholy, were reduced almost to a state of idiotism, and in which Galvanism seemed likely to be attended with the greatest benefit. In the last public sitting of the Institute of Bologna, at which I was present, I announced the complete cure of two lunatics performed in the Public Hospital, in the presence of many of the medical pupils, and with the assistance of the professors who superintend that establishment, and who are now employed in confirming my method on other patients. I am well aware that two cures are not sufficient to make operations of this kind be admitted as general remedies; but they ought to encourage physicians to prosecute this subject in order to ascertain how far Galvanism can be considered as of utility in such cases, and to endeavour by their labours to fill up the vacuity which still exists in this part of medicine. I am ready to acknowledge that great caution ought to be employed in performing such experiments; and for this reason I purpose, in the third part of the present work, to lay before the reader the reflections and observations of various ingenious men who

have

have made this department of Galvanism an object of their researches.

But before I proceed further, I think it necessary to deduce from what has been already said a few general corollaries.

COROLLARY I.

The muscles are affected by the action of the pile in a much more powerful manner when they are laid entirely bare, and when the arc is made to penetrate to a considerable depth in their substance.

COROLLARY II.

These convulsions are increased in proportion to the number of the points of contact between the arc and the muscle.

COROLLARY III.

In many cases, muscular contractions are obtained by forming an arc from one muscle to another.

COROLLARY IV.

Muscular contractions are almost always speedily obtained by the pile, even when the means proposed by Haller fail to produce them.

COROL-

COROLLARY V.

The heart, which, according to Haller's principle, is the first muscle that receives life and the last to lose it, in comparison of the other muscles, can with difficulty be made to feel the influence of the Galvanic action; while the other muscles always retain, a long time after death, that vital force which it has never been found possible to excite but by the impulse of Galvanism.

COROLLARY VI.

The partisans of Haller, to excite these contractions, often employ stimulants, which alter the texture of the muscular fibre, and destroy its continuity; an inconvenience which may be avoided by applying Galvanism.

COROLLARY VII.

As the kinds of apparatus before mentioned are not applied to the spinal marrow alone, but to the different nerves of the animal machine, they may afford to the anatomist an experimental myology; by means of which he can render sensible to the eye the fixed and moveable points of the muscles, and the real extent of their action.

COROL-

COROLLARY VIII.

The experiments made on the bodies of persons who died a natural death, are of the greatest importance to physiology. I am strongly inclined to think that, by pursuing these researches more in detail, they will one day make us better acquainted with the character of the vital powers, and the difference of their duration, according to diversity of sex, age, temperament and disease, and even according to diversity of climate and to the nature of the atmosphere.

PART

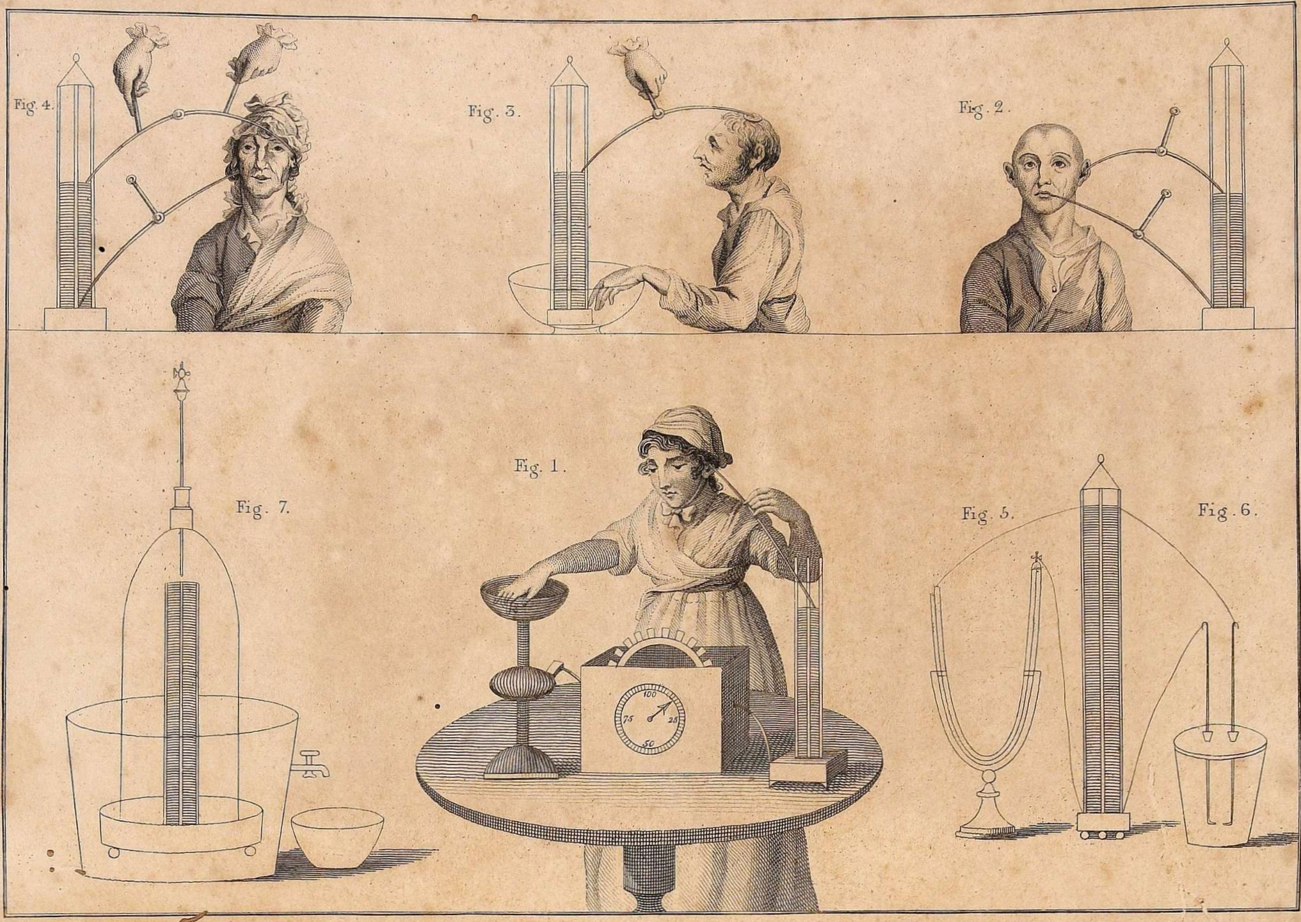
Fig. 4.
Fig. 3.
Fig. 2.
Fig. 1.
Fig. 7.
Fig. 5.
Fig. 6.
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PART THE THIRD.

ON THE POWER OF GALVANISM AS APPLIED TO MEDICINE.

If the doctrine of Galvanism have thrown considerable light on various parts of philosophy and chemistry, it gives us reason to hope that it may also be of benefit to medicine. The labours indeed of Galvani, whose most ardent desire was that his discovery might be rendered useful to mankind, were at length directed to this object, and his wishes now begin to be realized; but I must request the reader not to be too sanguine in his expectations, or to imagine that I here mean to entertain him with a long series of wonderful or extraordinary cures performed by means of Galvanism. I have no intention to decorate the discovery of my late uncle with false glory. Though I possess neither the same depth of knowledge, nor the same superiority of talents, I have always endeavoured to imitate his moderation and prudence in the application of his theory. I am fully convinced that much still remains to be done, in order to discover the best methods of employing this new agent; and that the facts re-

specting it, though numerous, have not been reduced to principles sufficiently certain and satisfactory. There are, nevertheless, some results and observations exceedingly curious, which, if confirmed by new experiments and researches, may enable us to obtain convincing proofs of its utility. New facts, however surprising, are not to be despised merely on account of their being different from any before observed. Those who reject them, as some have done, ought first to show that they are inconsistent with the principles of sound philosophy. Guided by these reflections, and desirous of contributing, as far as possible, towards the illustration of a new and very obscure subject, I shall lay before the reader the result of my researches in the following articles.

SECTION I.

Advantages which the medical administration of Galvanism has over that of common electricity.

Several reasons have induced me to prefer the medical administration of Galvanism by means of the pile, to that of artificial electricity. In regard to the action of the common electric machine, the difficulty of calling it forth properly during damp weather; the time required for exciting it; the necessity of charging the jar every time it is applied to the patient, are so many obstacles which render the administration of common electricity inconvenient; and on this account it is now much less used than formerly. On the other hand the pile, according to the observations which I have had an opportunity of making, acts in a uniform manner; is not sensible to the effects of moisture; and forms a sort of Leyden flask, which has a continued action that may be a long time employed. It may be considered as an apparatus, which in itself contains a series of jars charged in succession with different degrees of electricity.

I might here enumerate many differences which are found between the administration of Galvanism and that of electricity. If electricity be administered to a patient di-

rectly from the conductor, he receives very little of its action; if a shock be given with the Leyden flask, the action is exceedingly strong, but not permanent. On the other hand, the pile has a strong and continued action, and occasions a powerful circulation of the Galvanic matter, which after some time produces very striking effects on the animal fluids. I might here refer, by way of example, to the experiments made with Galvanism on the blood, the bile, and the urine, which gave results I was never able to obtain by common electricity. I shall mention, in the last place, the great convenience of the pile, and the short space of time in which Galvanism may be administered by it to a great number of persons; and this advantage is sufficient, cæteris paribus, to render it preferable in this respect to common electricity.

SECTION II.

Application of Galvanism to the organs of hearing and of sight.

By applying Galvanism to different parts of the face, a flash of light is excited in the eyes, which is stronger or weaker according to the nature of the parts to which it is directed. These organs, though delicate, are always affected in such a manner that the mechanism of the eye sustains no injury from the metallic arcs when they are made to communicate with only two plates of different metals. I received an account of this phænomenon at Milan, several years ago, from the celebrated Volta, who produced it by applying a conductor of zinc in such a manner, that one end of it touched the bulb of the eye, and the other the tip of the tongue armed with a plate of tin. In administering Galvanism for diseases of the eyes, it is much better to employ the pile. To excite the appearance of a flash of light, it is not necessary that the eyes should be open; it takes place even if they are shut, and covered with a bandage in a darkened apartment. Some of the partisans of Euler, perhaps, may here maintain, that this phænomenon of Galvanism is a deception, and that no light can be really excited in such cases, as the production of light depends on the emanation

of

of rays from a luminous body which penetrate into the interior part of the eye. But without entering into the examination of this question, which is foreign to my principal object, I shall only observe, that long before the theory of Galvanism was known, the celebrated Darwin had found that when the eyes are shut, there are certain internal stimulants which are capable of producing the appearance of light and of colours.

In no case is the difference between Galvanism and electricity more apparent than in the application of the former to the sight. Very little benefit had been obtained by means of a metallic point inclosed in a glass tube, which directed the electric charge from a jar to the cornea; and besides, the shock of a Leyden jar applied directly to so delicate an organ as the eye could not but be attended with danger. Galvanism applied to the exterior part of the eye, in the same place where a stream of electricity would have had no influence whatever, has always produced a certain effect on the organ of sight, as is proved by the following experiments.

EXPERIMENT I.

If you touch with one hand the bottom of the pile, and at the same time apply to the summit different parts of the face moistened with salt water, a flash of light will be excited in the eyes. The same result will be obtained, if instead of touching the bottom of the pile with the hand you touch

it with the sole of your foot. No flash of light is observed when the Leyden flask is employed in the same manner.

EXPERIMENT II.

Having observed the preceding phænomenon in myself, and excited it in others, I was desirous of proving it in regard to several persons at the same time during the course of my public lectures, and for that purpose I made use of the following apparatus. I arranged two metallic plates in a horizontal position, at the distance of nine inches from each other, so that six persons with their hands dipped in salt water could touch the lower plate, and the upper one with the tips of their tongue. A charged Leyden flask placed between the two plates being then discharged, the whole of the persons experienced a violent shock, but perceived no flash of light. It is well known however that, when a similar arc is formed with the interposition of a very strong pile, a flash of light is constantly observed; though the force in the second case is much less than that excited by the explosion of the Leyden flask. The same result will be obtained by bringing the upper plate into contact with the nose.

EXPERIMENT III.

As my pupils took much interest in this research, some of them suspected that the light of the apartment might perhaps

haps have effaced that excited by the electricity. I therefore made the apartment entirely dark; and one of them taking a Leyden flask applied it to the point of the nose of another person with whom he was in communication, by laying hold of him with the other hand. By these means a very strong shock was given, but no flash of light was observed. This experiment was repeated, making the person who received the shock of the Leyden flask to remain some time before in the dark, that his eyes might be better enabled to perceive any faint light that might be excited: but the result was still the same. To those who refer Galvanism to the common laws of electricity, it will be difficult to comprehend the cause of the different action exercised by the latter on the organ of sight. But as it is not my intention at present to enter into any discussion on this subject, I shall leave it to philosophers to assign a reason for this phænomenon, and only observe, that the properties above indicated will be sufficient to authorize medical practitioners to prefer, in certain cases, the administration of Galvanism by the pile to that of common electricity.

Before I proceed directly to the medical administration of Galvanism to the organ of sight, I think it necessary to distinguish four classes of blind persons whose cases ought to be considered separately.

The first belongs to those who from their birth have been deprived of the valuable blessing of sight.

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The second comprehends those become blind in consequence of some great læsion, or some derangement in the solids or in the fluids which constitute the mechanism of the eye.

The third, those who have become blind by some morbid action, though the mechanism of the eye has been little affected, and though no impediment has occurred but in regard to the action of the optic nerve.

The fourth class comprehends those who, though not actually deprived of sight, have it much weakened in consequence of disease, or of some other cause.

The administration of Galvanism does not hold forth much hope of a cure to persons belonging to the first two classes. I however resolved to attempt some experiments on this subject at Bologna; but though there were a great many blind in that city, I found that they had become so by the malignant influence of the small pox. This observation will, I hope, be of service to the pursuits of the celebrated Dr. Jenner, and of all those who exert themselves to promote the beneficial practice of vaccine inoculation.

Being deprived, at Bologna, of any opportunity of trying the effects of Galvanism in cases of persons born blind, I galvanised several who had lost their sight at a very early age. I first applied the Galvanism to the arms of five blind

persons, some of whom had lost their sight thirty, and others forty years and even more. By this method they were familiarised with the idea of its mechanical action, and learned to distinguish it from every other sensation. I then applied Galvanism to the lips, and to the tip of the nose, in a darkened apartment; but in three instances only the patients had a real perception of light, to which they had been so long strangers. I then applied it to cases of amaurosis, and at first had confident expectation of effecting a perfect cure. One of my patients was a woman, whom this disorder had deprived of the sight of one eye, while that of the other was much weakened. After administering the Galvanism different ways, I observed that the eye totally blind began to have a perception of light, and that the sight of the one which was weakened became much stronger. I then took a book, which I held at a considerable distance from her; and removing it gradually further as the Galvanism was administered, I observed, in consequence of this method, that the patient's sight daily improved. But I must freely confess that the success obtained, though at first flattering, was not of long duration; and that when the Galvanism was discontinued a great deal of the benefit which had been obtained was again lost. On this account, I was discouraged from administering Galvanism any more in such cases. I am however of opinion, that by varying the method of administration it may be attended with some utility. I shall here observe, that having once had occasion to administer common electricity in a similar case of amaurosis, I was never able to excite

excite the perception of a flash in the eyes of the patient, though the electricity was applied directly to the eye itself.

I have had few opportunities of applying Galvanism in diseases of the organs of hearing. Besides, I thought it almost needless to try a method which had been already brought to a state of perfection by some of the most celebrated professors of Germany and Berlin. I admired in particular a very ingenious machine, invented for that purpose by a German philosopher, and lately constructed in England by Mr. John Cuthbertson, an eminent philosophical and mathematical instrument maker, and celebrated for having constructed the large electrical machine of Harlem. The apparatus consists of a metal lever, which by means of certain wheels and machinery rises and falls every minute or second, and at each time of falling forms a communication between certain parts of the patient and the pile. In consequence of this arrangement, the interrupted action of the Galvanism is renewed every time that the communication between the patient and the pile is re-established. Before my departure from London, I made several changes and improvements in the usual construction of this apparatus, in order to give it as much simplicity as possible. The following is the manner in which I caused it to be constructed lately for my own use. One extremity of the lever which forms the communication (Plate III. fig. 1.), is fixed to the base or negative end of the pile, and the other terminates in a small hammer, so placed as to strike a bell, which by means of a

bason of water is in communication with certain parts of the patient, while an arc extends from the patient to the summit or positive end of the pile. In consequence of this arrangement, every time that the small hammer strikes the bell the Galvanic action of the pile is repeated. In cases of deafness, I cause the patient to hold in one hand an insulated metallic arc, one end of which is brought into contact with the affected ear, and the other with the positive end of the pile; and to immerse the other hand in a bason of salt water placed above the bell. When this disposition has been made, the wheel-work is turned round, which gives motion to the lever; and every time that the hammer strikes the bell, a communication is formed between the positive and negative ends of the pile: consequently there will then be a circulation of the fluid, and the Galvanism will exercise a direct action on the organ of hearing. The apparatus, constructed in this manner, appears to me to be reduced to great simplicity: and therefore I propose to extend the use of it, by employing it to administer medical Galvanism to other diseased parts of the body.

Before I conclude this article, I must suggest a hint respecting the application of Galvanism to diseases of the teeth, founded on information communicated to me on this subject by Mr. Fowler, an eminent dentist of London. When the caries is concealed from the sight, Mr. Fowler employs the following method to discover the affected tooth. He first insulates the patient; and having put into his hand the electric

tric chain, he applies a small piece of wire to the *dens sapientiæ*, drawing it gradually over its surface: he then applies it to the next tooth, repeating the operation, and proceeds in like manner with the rest till he comes to the diseased tooth, which discovers itself by a violent pain producing an involuntary commotion in the body. It is always remarked, that when this tooth is extracted it exhibits a carious part not before visible. This method, therefore, is of great importance, as it frequently happens in such cases that the dentist, not being able to distinguish the diseased tooth from the rest, is obliged to draw some that are sound before he can discover it.

SECTION III.

Application of Galvanism in cases of asphyxia and drowning.

I mentioned in the second part of this work the great influence which Galvanism has in cases of asphyxia, and the preference which ought to be given to it in comparison of other stimulants. Though the observations offered in that part are sufficient to prove my proposition, I shall add to them the following experiments:

EXPERIMENT I.

Some dogs and cats were immersed in a large pond till they gave no external signs of respiration, or of muscular motion; and Galvanism being immediately administered to them, according to the methods already described, they were sometimes restored to life. I make use of the term 'sometimes,' because, if animals are immersed in water for a longer period than their organization can bear, and if the vital powers are really destroyed, it is evident that it will be impossible to restore them to life by any physical process whatever. I obtained the same results from to animals thrown into a state of asphyxia in different ways.

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EXPERIMENT II.

Having applied Galvanism to the trunk of a dog, in the *Hôpital de la Charité* at Paris, air seemed to escape from the tracheal artery on every application of the arc. Being requested to repeat and confirm this interesting experiment, I found myself under the necessity of sacrificing a new victim to my Galvanic researches. As it was necessary to examine the phænomenon while the body was in that state of vitality most proper for the observation, I exposed the trunk of another dog recently killed to the Galvanic action; and having placed a taper near to the tracheal artery, it was extinguished twice in succession by two applications of Galvanism. By repeating this experiment, in Mr. Wilson's anatomical theatre, Great Windmill-street, and in the theatres of Guy's and St. Thomas's Hospital, London, I found that the taper could be extinguished a greater number of times.

These experiments give me sufficient reason to hope that Galvanism may be administered with some advantage in cases of drowning. But as I never had an opportunity of trying the effects of this stimulant in such cases, I have requested several medical practitioners to pay attention to this application of Galvanism, which may be of the utmost importance to the cause of humanity. I have already mentioned that the method which I propose is exceedingly simple; that no anatomical operation whatever is required; that it is attended with no danger; and that the possibility of saving the

the life is in every case respected. Nothing is necessary but to immerse in salt water one of the hands of the person subjected to the operation, and then to apply the Galvanic current to one of the ears and to the surface of the salt water.

Dr. Lettsom, a very zealous member of the Humane Society, having been present at some of my experiments, I requested that he would recommend the application of Galvanism in cases of drowning. He assured me that he would use his endeavours to cause my method to be tried, and I had several conferences with him on the subject, that I might communicate to him such information as might tend to facilitate the application of it. As the Galvanism in such cases ought to be administered with great promptitude, we agreed that the apparatus of the trough is preferable to that of the pile; and we contrived the plan of a portable box to contain a trough, two arcs, and a solution of common salt. Such an apparatus will be exceedingly convenient, and may easily be employed in all cases of drowning and of asphyxia.

SECTION IV.

Galvanism applied to the cure of melancholy madness.

Hospitals for lunatics present a spectacle which must excite compassion in every breast not void of humanity, as they are in general crowded with unfortunate beings, useless to themselves and dangerous to others; while little hope is left of their being ever restored to society. This is the case in particular with persons subject to melancholy madness; on some of whom I tried last year the effects of Galvanism. In consequence of a long series of painful and disagreeable experiments made on myself, I was induced to entertain great hopes from this remedy. I applied it to my ears and to different parts of the head, in order that I might form a just estimation of its power and influence on the brain. It is well known that the strength and energy of the operations of the mind depend on the state of the functions of the brain. It is well known also, that a violent fall, or strong impression on the head, has often occasioned great variations in the intellectual faculties. By such accidents some have entirely lost the power of imagination; while, on the other hand, some have acquired very great talents, or emerged from a state of complete stupidity. These considerations gave me reason to hope that the power of Gal-

vanism might perhaps be able to produce a salutary change in the brain in cases of melancholy madness. I communicated my thoughts on this subject to the physicians who superintend the hospital for lunatics at Bologna. They approved of my ideas, and gave me every assistance in their power to enable me to prosecute my researches. In their presence, and under their direction, I administered Galvanism to several lunatics, applying it different ways; and the result was the complete cure of two who had laboured under melancholy madness. As the method I followed in both cases was nearly the same, I shall here give a description of it, as it may serve as a general rule for the administration of this remedy under similar circumstances.

Louis Lanzarini, of a phlegmatic temperament, twenty-seven years of age, and a farmer by profession, fell into a state of deep melancholy, which first announced itself by an attack of fever; in consequence of which he was conveyed to the public hospital of St. Ursula on the 17th of May 1801. When he arrived there, he began to complain of the treatment he had received, and to show great uneasiness; by which means his melancholy increased so much, that it at length degenerated into real stupidity. While in this state, Professors Gentilli and Palazzi were so kind as to allow me to administer Galvanism to him; which I did in the presence of these physicians, and of several medical students who attended the hospital.

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I had provided for this purpose a pile composed of eighty pieces of silver and zinc, and I at first administered the Galvanism gradually, forming the arc by means of the hands. Lanzarini, in a state of the utmost dejection, viewed the apparatus and the company present with his eyes fixed and motionless. When interrogated by the physicians and myself in regard to the origin of his malady, he gave laconic and confused answers, which seemed to indicate a great degree of stupidity and derangement. I first moistened his hands, and formed an arc with the pile at different heights, to accustom him to endure the action of the apparatus. No change, however, was produced in the patient by this operation. I then repeated the experiment, placing his hands, moistened with salt water, at the bottom of the pile; and conveying an arc from the summit of the pile to different parts of his face, moistened with the same solution. A change was soon observed in the patient's countenance, and his whole demeanour seemed to indicate that the degree of his melancholy was somewhat lessened. The experiment was repeated several times with the same success; which seems to prove that Galvanism absolutely exercises an action in such diseases. The patient being interrogated next day, asserted that he had felt no inconvenience from the application of the Galvanism; and this account was confirmed by the keepers, who had been desired to give a report of the least change that might take place. Similar results were obtained by gradually administering the action of the pile with greater force for several days successively, and we soon

began to observe that it produced a very striking effect. The patient, on touching the apparatus, seemed to acquire new spirits; a smile appeared on his countenance, and a complete change took place in his eyes as well as in every feature of his face. Instead of showing any aversion to the pile, he readily obeyed whenever he was called to undergo the operation; and his whole conduct indicated that he found relief from the influence of the unknown agent which it excited. He began to converse with more readiness, sometimes respecting the machine, and sometimes on the flash of light which appeared in his eyes when the arc was applied; and on that account we conceived the most flattering hopes of a complete cure. The result of this operation induced me to administer the Galvanism even to the substance of the brain; being convinced, as I have already remarked, that the Galvanic fluid by this method of application exercises its action with greater energy. I communicated my design to the Professors of the hospital, and with their approbation began to try the effects of a pile composed of fifteen plates of copper and zinc. I formed an arc from one of the hands to one of the ears, and then from one ear to another, having first moistened them with a solution of muriate of soda. I increased the number of plates of which the pile was composed, and found that the patient was always more or less affected with a momentary impression exceedingly painful, which however seemed in the end to produce a good effect. When Galvanism was administered in this manner, I did not neglect to continue the application of the other method at

the same time; and I found that the progress of the cure became more rapid. But as I observed that the action of Galvanism on the ears was sometimes too violent, we thought proper to apply it in a more moderate and less dangerous manner. Several persons having been induced through curiosity to try this action, the result, besides a violent commotion of the whole substance of the brain against the skull, was a state of watchfulness which continued several days running, and which I experienced myself as well as others. We then conceived the idea of shaving the head above the suture of the parietal bone (Plate III. fig. 3); and having moistened the shaven part with salt water, a piece of gold or silver coin was placed over it. The patient then touched with one of his hands the bottom of the pile, and at the same time an arc was established from the summit of the pile to the metallic armature placed on the head. By this arrangement the action of the Galvanism was rendered more moderate; the patient endured it for a long time, and seemed to be greatly relieved by it. I have always united this method with external application to different parts of the face, and have observed such sudden changes in the looks as seemed to announce a considerable abatement of the disease. Some of the physicians of Bologna, Professors Brugnatelli and Zola of Pavia, and several other foreigners, examined and confirmed the permanency of this effect. The patient, therefore, not only got the better of his melancholy, but began to relish his food, and at length recovered so much

strength

strength that the physicians of the hospital thought nothing further was necessary to complete the cure.

No other remedy besides Galvanism was administered to him, lest the effects should be so confounded as to render it impossible to tell to which the cure ought to be ascribed. Two days, however, before he left the hospital a little blood was taken from him; as it was conceived that this operation might contribute to render the cure more certain.

On his leaving the hospital I carried him to my house, that he might be fitted by proper nourishment for resuming his former occupations. He remained with me eight days in the quality of a domestic, during which time he was exceedingly tractable, and performed his duty with great care and attention. I had several conversations with him, in the course of which I learned that his father, Fabian Lanzarini, had been attacked by the same disease, and that he had been admitted into the same hospital, where he died on the 12th of June 1790. By inspecting the registers of the hospital, I found this account to be perfectly correct.

Agreeably to the principles already established, in regard to the treatment of madness, I advised Lanzarini to spend the rest of his life at a distance from his native country, lest, having continually before his eyes those objects which had occasioned his disease, it might recur with double violence.

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But though he had given me several proofs of his docility, I found it impossible to persuade him to make this sacrifice. A kind of *nostalgia*, perhaps, attached him to his former master, to whom he returned, after paying a visit to the curé of the parish. The latter, when he first saw him, imagined he had run away from the hospital; but by his conversation he was soon convinced of his being completely cured. After this period, I obtained a regular report respecting his behaviour and the state of his health, from the above curé, and from the person who had paid all the expenses of his residence in the hospital; and I learned, with great satisfaction, that he continued to enjoy good health, and to exercise his usual employment.

By the same treatment I cured, of a similar disorder, Charles Bellini, a labourer, who was restored to society in a shorter space of time, because the affection was not so violent as in the preceding case. The phænomena which took place when the patient was subjected to the action of the pile, when the Galvanism was applied to the brain, and during the whole progress of the cure, were nearly the same. I must, however, freely acknowledge, that two cures are not sufficient to establish the application of Galvanism as an universal remedy in such cases. But on this account it ought not to be rejected: at any rate it deserves further examination; for it is well known that all remedies require certain conditions before they can perform their effect. I have therefore several times found it impossible to obtain the same

result

result in other patients afflicted with melancholy madness, to whom I administered Galvanism; and in cases of raving madness I have even found it dangerous. In some instances, melancholy madness derives its origin from a certain general constitution of the animal machine, or from some great alteration in the brain; and it is evident that in such cases the action of Galvanism would be of no avail. But if the derangement of the intellectual functions depend only on some humour intercepted between the membranes and other parts of the brain, there is reason to hope that Galvanism, if prudently administered, may be attended with great benefit. The real cases in which it may be administered with success, can be ascertained only by experience. I must observe also, that the method of administering it is not yet reduced to that state of simplicity, which is necessary before it can be brought into regular use in large hospitals. The physicians, under whose care they are placed, have in general a great deal of private practice, and cannot conveniently attend to operations which require a continued labour for several months. Besides, the novelty of the remedy is sufficient to excite a clamour against it, and to awaken the prejudice of the assistants, who will even wish to proscribe it before it has been tried. For this reason, I think it necessary here to request, that those who preside over establishments destined for the reception of such patients would turn their attention to this subject, and endeavour to reduce the method of applying Galvanism to the utmost simplicity of which it is susceptible, in order that it may

may be fit for being introduced into large hospitals. As the patient often shows an aversion to this strange remedy, it will be necessary to encourage him by every means possible. Sometimes on observing the flash of light, when the Galvanism is communicated, he cries out and is frightened; imagining that he sees a devouring fire ready to consume him, and on this account refuses to submit again to the operation. It will, therefore, be proper to conceal from him the apparatus, or to make a person show him the pile some time before as an object of amusement, and in this manner to prepare him for receiving its action. It will be of benefit also sometimes to modify the action of the pile, and to render it more moderate by a different method of application. In the case of female patients I have found the result the same, when the Galvanism, instead of being applied directly to the interior part of the ears, was directed externally to the gold pendents (Plate III. fig. 4.).

By considering the course generally pursued in curing melancholy madness, hints may be suggested for an useful application of Galvanism in that disease, and data may be obtained sufficient to establish the different modes of application best fitted to the different cases. These ideas have engaged a good deal of my attention; and when I have finished the observations I have been for some time collecting, I flatter myself that I shall be able to communicate to the public some interesting information on the subject.

In some cases of madness, as I found it impossible to ap-

ply Galvanism to the hands, which were confined, I employed the action of an arc directed to the mouth (Plate III. fig. 2.), while another proceeded to one of the ears; or I applied a piece of money to the head, and communicated the Galvanism by the method already described (Plate III. fig. 3.).

It will even be necessary to try the effect of the Galvanic current, sometimes continued, and sometimes interrupted by means of the apparatus employed for diseases of the organs of hearing. It will be proper, in many cases, to combine moral with physical treatment, and not to neglect the other methods already known and practised, which may be used as very convenient auxiliaries.

SECTION V.

General reflections on the action and influence which Galvanism, considered in a medical point of view, exercises on the animal œconomy.

I shall not here speak of the variation in insensible perspiration, and of the increase of circulation, which, according to the observations of several physicians, are found to be produced by Galvanism. Similar phænomena, as is well known, take place during the administration of common electricity. I shall therefore confine myself to those effects which hitherto have been produced only by the action of Galvanism. I shall observe in the first place, that Galvanism, as already shown in the Experiments detailed in the Second Part, is capable of effecting a separation of the fluids, and even sometimes of protruding the fæcal matters from the body. In the case of the decapitated malefactor, I found that when the arc was applied to one ear and to the lips, a very sensible portion of saliva was discharged from the mouth: This observation was confirmed at Genoa on the head of an ox, and in several other places on the heads of sheep. The phænomenon of the extrusion of the fæcal matters from the trunk of an ox, by means of Galvanism, was

observed

observed also by Professor Mojon of Genoa, and his brother, to take place in human bodies. Considering the animal fluids separately, I have found that very great variations are produced in them by Galvanism. But before I give an account of the experiments which I made on this subject, I shall describe the apparatus I employed.

The animal fluid destined to be exposed to the action of Galvanism is put into a glass vessel (Plate III. fig. 6.) covered by a wooden lid, having in it two holes equally distant from the centre. Two wires, one of brass and the other of plated copper, the upper extremities of which are bent into the form of a hook, pass through these holes in such a manner, that the lower extremities of them reach nearly to the bottom of the vessel, where they are bent at right angles, so that only a very small interval is left between them. The upper extremity of one is made to communicate with the bottom of the pile, and the other with the summit. In consequence of this arrangement the Galvanic fluid is obliged to traverse the animal fluid, by which means it exercises an action on it according to the distance of the wires, and by its action separates from its different strata sometimes one principle and sometimes another; and this secretion will be effected with more ease and in greater abundance, according as the action of the pile is stronger, and the capacity of the conductors more considerable.

EXPERIMENT I.

Having put into glass vessels four ounces of blood recently drawn from the vein of a person in good health, I left one of them exposed to the contact of the atmospheric air, and subjected the other to the action of the pile. In both these portions I observed a speedy coagulation of the crassamentum, and at the end of twenty-four hours the serous part was separated. The blood exposed to the action of the pile adhered so strongly to the two wires immersed in it, that it was difficult to separate them from the clot which was thus suspended in the aqueous fluid, but in the other vessel the clot remained at the bottom.

EXPERIMENT II.

I put two equal portions of bile, still warm, taken from the gall-bladder of an ox, into two glass vessels, exposed one of them to the contact of the air, and subjected the other to the action of the pile. After ten hours had elapsed, I observed that the bile in the latter had become so opake as no longer to afford a passage to the light; while the other portion, exposed to the atmosphere, retained its transparency and colour. I observed also a considerable disengagement of air, the nature of which I have not yet had an opportunity of examining.

EXPE-

EXPERIMENT III.

I took four ounces of urine, voided by a man in good health, exposed it to the action of the pile, and at the end of twenty-four hours found that the greater part of its constituent principles was separated. A portion of them was collected around the wires in such a manner as to form cylindric bodies of a considerable diameter, of which the wires were the axes. As the mass of the attracted matters increased, a portion fell to the bottom by its own weight. The cylinders were soon entirely destroyed, and the substances which formed them were precipitated by the least shock given to the vessel. I repeated this experiment lately in Mr. Wilson's anatomical theatre in Windmill-street; and I observed, at the end of eighteen hours, a great quantity of the moleculæ furnished by the urine adhering to the two wires. But at length, not being able to withstand the effect of its gravitation, it began to fall down, forming a sort of wedge, the apex of which was at the surface of the urine, and the base at the bottom of the vessel.

EXPERIMENT IV.

Instead of putting the urine into a common apparatus, if a glass syphon with two platina wires be employed (Plate III. fig. 5.), the urine becomes limpid on the one side, and turbid on the other. The substance detached from the urine afterwards

afterwards appears in the form of flakes, which are attracted by the platina wires. I observed this phænomenon for the first time in the company of those celebrated chemists Fourcroy and Vauquelin, while they were performing Galvanic experiments in their own laboratory.

EXPERIMENT V.

The substance above mentioned, which was precipitated to the bottom, when separated by filtration and dried, weighed about the fourth of a grain, and the fluid separated from it was of a greenish colour. On examining the earthy deposit of this urine, we obtained sulphate of lime by adding to it sulphuric acid.

EXPERIMENT VI.

Having exposed to the action of the pile, in the same manner, four ounces of urine voided by a person with jaundice, I obtained an earthy sediment, the weight of which was nearly equal to that above mentioned. The liquor separated from it was transparent, inclining a little to black. By the same chemical process I obtained sulphate of lime, though the sediment of the urine was somewhat dark, and afforded a portion of carbon and bile which inflamed in the fire.

EXPE-

EXPERIMENT VII.

Having repeated the above experiments with different kinds of urine, I observed in general, that Galvanism, by a peculiar attraction, separates from urine the sulphates and muriates united to a portion of the bile, and also to carbon, which in a great part are precipitated to the bottom of the vessel: the other part, which remains attached to the wires, exhibits a regular saline crystallization, of so singular a form that it seems worthy of becoming an object of further research to chemists.

The examination of urine voided by persons labouring under different kinds of disease, seems to be an object sufficient to excite the curiosity of physicians. To expose to the action of Galvanism artificial aëriform fluids, analogous to those which act a part in the animal œconomy, might also be attended with advantage. For this purpose it will be convenient to employ the apparatus represented in Plate III. fig. 7, which was lately constructed, with great precision, by M. Dumoutier at Paris. The whole artifice consists in a vertical metal tube, which can be raised up or pushed down. It is furnished with a stop-cock, and the tub in which the apparatus is placed has another. If the inverted bell be filled with water, and connected, by means of the vertical tube, with the apparatus which is to supply the gas, on opening the lower cock, the water will descend in the bell,

and

and its place be supplied by the gas intended to be subjected to experiment.

From all the observations hitherto made, there seems to be reason to conclude, that the effects produced on the animal œconomy by common electricity and by the Galvanic pile are different. The phænomena of artificial Galvanism give us some right to suppose, that a similar action is exercised by the Galvanic fluid circulating in the fluids and in the organs of living animals. In this point of view Galvanic researches may one day throw great light on the nature of secretion; and it may perhaps be found necessary, when remedies are administered, to take its influence into consideration: for it is possible that the action of these remedies in the animal œconomy may depend on the establishment of such an arc between the system of the nerves and that of the muscles, as may not alter the natural state of the Galvanic fluid proper for the constitution of the individual to whom these remedies are administered. All this however is mere conjecture, and must be classed with many other things in the theory of Galvanism which are still involved in obscurity, and which we can hope to see explained only by new researches and new experiments.

Taking a general view of this Third Part, I must observe that the administration of Galvanism, when the above experiments are carefully examined, seems to appear in a much

more advantageous point of view than before. I have indeed proved :

1st. That Galvanism, on many occasions, exercises an influence different from that of common electricity, and that it may be administered in various cases with great ease and safety.

2d. That the action of Galvanism manifests itself by a sensible attraction between the nervous and muscular parts; which seems to confirm the hypothesis of Humboldt, who supposes a Galvanic atmosphere peculiar to these parts when in a state of perfect vitality.

3d. That the strong impression made by Galvanism on the brain seems to explain its power on the organ of hearing; and therefore the physicians of Berlin, and other parts of Germany, are entitled to great praise for their researches on this subject.

4th. That though medicine is capable of affording considerable aid in cases of drowning and of asphyxia, it presents us with no means so powerful as Galvanism. The experiments made at London, Jan. 17, 1803, on the body of Forster, executed for murder, have fully convinced me of the activity of this stimulant.

5th. That in cases of melancholy madness, when other

remedies fail, Galvanism may be employed with the greatest hopes of success, provided the disease does not proceed from a vitiated constitution, or a general derangement in the animal machine.

6th. That the current of the Galvanic fluid produces a great alteration in the animal fluids; separates a great many of their principles, and produces this effect in a particular manner in urine.

It is however much to be wished, that in addition to the knowledge already acquired in regard to Galvanism, some convenient method could be discovered of increasing or lessening its action on the animal fluids; by which means the advantages of the medical administration of this subtle agent would be rendered more certain and more effectual.

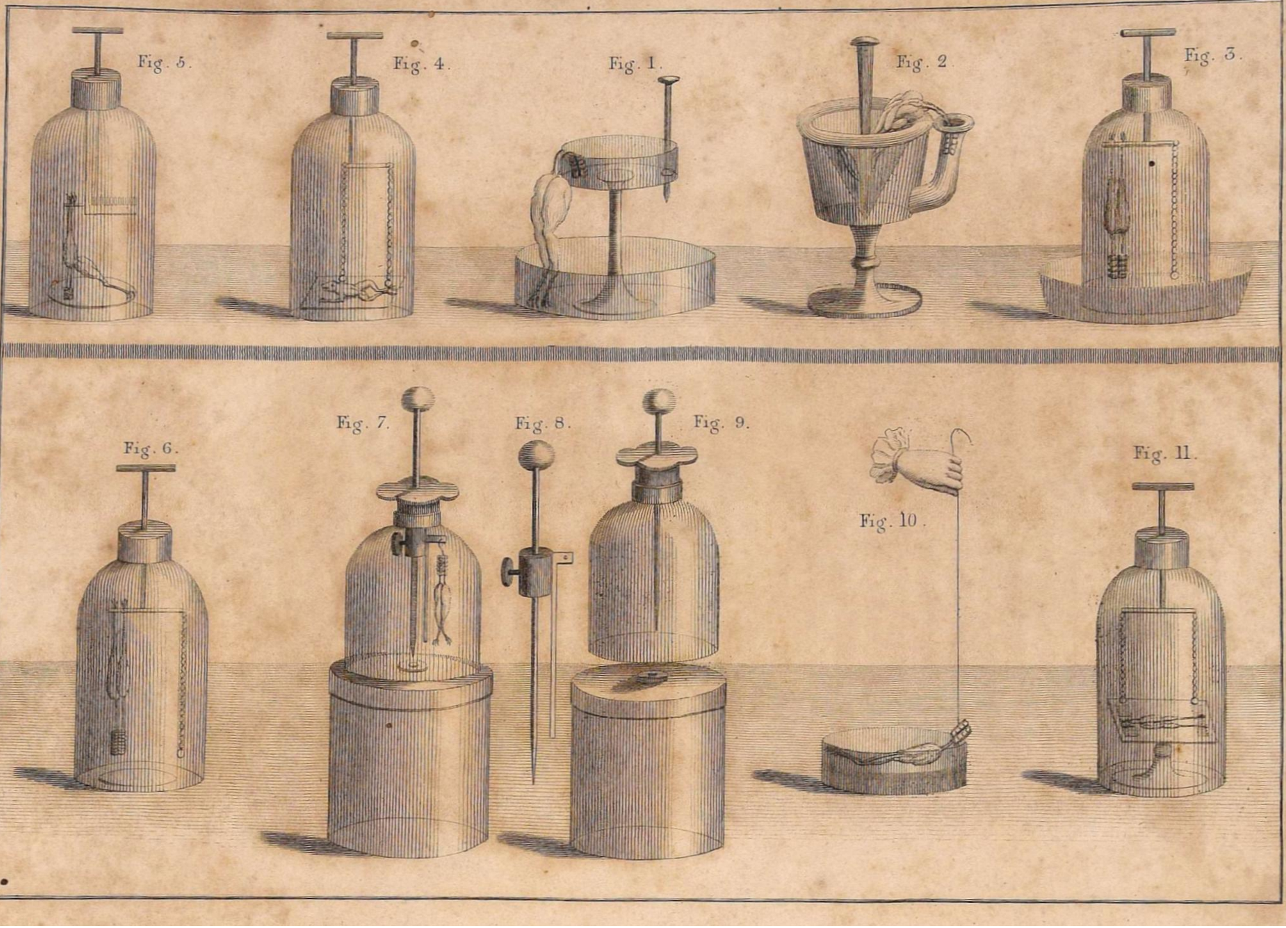
Fig. 5.
Fig. 4.
Fig. 1.
Fig. 2
Fig. 3.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.

DISSERTATION

ON

ANIMAL ELECTRICITY:

READ IN THE

INSTITUTE OF BOLOGNA,

IN THE YEAR 1793,*

BY J. ALDINI.

I. WHILE our Academy was congratulating itself on the progress made by the doctrine of animal electricity, its exultation was in some measure checked by an objection brought against it, which did not attack any one part of it, but the whole theory. If the contractions in animal bodies, said its opponents, are produced merely by the electricity of metals, how degraded is that electricity which at first was supposed

* The title of the work from which this and the following dissertation are translated is, *Joannis Aldini de Animali Electricitate Dissertationes duæ.* Bononiæ 1794.

to

to reside in animal bodies, since it is now found to be subservient to the electricity borrowed from metals! I heard repeated objections of this kind while labouring under a severe indisposition; but being restored to health by the skill and attention of Galvani, I took the earliest opportunity to inquire after the success of his animal electricity, and at the same time promised him every assistance in my power in the prosecution of his researches, for which I always entertained a great fondness. He accepted my offer; and as I had now recovered my former strength and vigour, I was anxiously desirous to defend the cause of animal electricity, attacked and almost exploded, amidst a variety of contradictory opinions, and with this view to undertake a new series of experiments.

II. The theory of animal electricity had scarcely been proposed, when a suspicion was entertained that it might be produced by some external agent excited by the arming or by the arc. This suspicion Galvani endeavoured to obviate in various ways. By using an insulated arc, it was impossible that the person who performed the experiment could communicate any of his electricity to the animals. In preparing the frogs he employed no conducting bodies; he neither touched them with his fingers nor with a knife; he uncovered the muscles and nerves with idio-electric bodies, and still the usual contractions took place. Nay, Galvani carried his attention so far as to exclude even the air. Having immersed a frog with an insulated arc in a glass vessel filled with oil, and having made a communication by means of an

arc between the muscles and nerves, muscular contractions were immediately produced. This electricity, the animal being thus surrounded by idio-electric bodies, could not certainly be furnished by the atmosphere, from which it was entirely separated. While engaged in these experiments, the celebrated Spallanzani stopped a few days at Bologna on his way to Pavia, and during a short conversation which he had with Galvani on his new system, expressed some suspicion that the electricity observed in animals might be acquired from external bodies. After some discussion however on this subject, Spallanzani acknowledged that the experiment made on the frog immersed in oil was so conclusive, that nothing could be better calculated to satisfy all his doubts. I mention this circumstance, because the approbation of so eminent a man was of the utmost importance to the cause of the theory of animal electricity.

III. Having proposed the before-mentioned series of experiments, I was led into various reflections on this subject. As the same electricity exercises an extensive action in the different parts of animals—some becoming electric through an excess, and others through a deficiency of it—I could not comprehend why there should not be an electrical movement even when the end of a very long arc was covered by a non-conducting body; and why the same arc touched by the same person, in the same atmosphere, should sometimes become charged with a very small quantity of electricity, only capable of exciting motion in the legs of a frog, and some-

times

times with a large quantity sufficient to produce contractions in the leg of a lamb or a calf. This circumstance appeared to me to be involved in a considerable degree of obscurity. I conjectured also that internal electricity might have been implanted in animal bodies for the purpose of defending the animal œconomy, and protecting it against any injury which it might sustain from an excess of the external atmospheric electricity. Were not this the case, there might be reason to apprehend that the electricity of the clouds, in the time of storms, might attack the human body and destroy it.

IV. But though the suspicion of electricity being communicated from the air, or from the person who performed the experiment, was lessened or removed, there were still some who thought that the objection in regard to metals could not be obviated in the same manner. The idea generally entertained, that all the contractions ascribed to animal electricity were to be ascribed only to external electricity, proceeding from the armatures, and not to any electric virtue in the animals, seemed to be strengthened. Carradori, who had made this objection, afterwards altered his opinion, and became a strenuous advocate for animal electricity; but the celebrated Volta still entertained great doubts. These he communicated to me in a long letter, and they were afterwards published in the Pavian Journals. All his doubts were founded on this circumstance: heterogeneous metals are required to produce contractions; one of which becomes charged positively and the other negatively; and it is only

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in the act of restoring an equilibrium between them that muscular contractions are excited.

V. This simple and ingenious idea was no doubt highly captivating; but the experiments made by Galvani and by myself prevented me from adopting it entirely; for it is evident from Galvani's Dissertation, and from my own observations, that if the muscles and nerves of a frog be immersed in two vessels filled with water, contractions will be produced on the application of a metallic arc. Here then we have muscular contractions with one arc and with one metal. Living frogs subjected to the action of rarefied or of condensed air, and afterwards dissected in the usual manner, exhibit contractions without any armature, and merely by a silver arc applied to the muscles and nerves; and this is always the case, whether the frogs be large and strong, or small and weak. While engaged with these experiments, Galvani informed our academy that he had obtained contractions in large and vigorous frogs, which had been recently dissected, without the help of the air pump and without armatures, merely by the application of an arc. The same thing occurred to Carradori, who, though he at first entertained doubts on this subject, was afterwards convinced of the truth of the phænomenon. It still however appeared to me, notwithstanding the results obtained by Galvani and Carradori, that the animal electricity of frogs was excited in more abundance by the action of the air pump, even without the application of armatures.

VI. But that the series of experiments I had undertaken might be better calculated to establish the theory of animal electricity, and as it was difficult to find any of the solid metals homogeneous throughout, I had recourse to mercury, which, by the help of chemistry, may be brought to a very considerable degree of purity. For the greater convenience in the employment of this substance, I invented the following apparatus:—Two glass vessels are so arranged (Plate IV. fig. 1.) that the one stands above the other: the upper one is filled with mercury, and the spinal marrow of a prepared frog is placed in it; and by means of a hole in the bottom of it, which may be opened at pleasure, the mercury can be made to fall on any part of the muscles of the same animal placed in the lower vessel. The stream of mercury occasions convulsive movements to take place in the muscles; and yet in this case the mercury forms both the armature and the arc: consequently the electricity in both is the same, and can exercise no action. When contractions therefore take place, they cannot be ascribed to the electricity of the metal. Should it be said that the mercury as it runs down may excite electricity from the sides of the glass vessel, as is the case with the mercury in the upper part of a barometer, which, when in the least agitated, shines with an electric light, any doubt on this subject may be easily removed by substituting vessels of wood instead of those of glass.

VII. When these vessels are used, care must be taken to pay attention to one circumstance, which may prevent the

the success of the experiment. As the spinal marrow is exceedingly light, it will float on the surface of the mercury; whereas it ought to be immersed in that metal. It will be necessary therefore to press down the spinal marrow below the surface of the mercury, by means of a glass rod, or any other non-conducting body, so that it may be entirely immersed; otherwise some irregularity may take place, and the experiment fail. To perform it with the greatest convenience, provide a glass vessel consisting of two branches in the form of a syphon, as represented in Plate IV. fig. 2., one of which is wide, and has its edges reflected inwards, so as to form an inverted cone, ending in an aperture that can be opened and shut at pleasure. If mercury be poured into the narrower branch, so as to fill the interior part of the vessel around the inverted cone, it will not be able to rise up into the latter, until the aperture in its apex be opened. When this arrangement has been made, immerse the spinal marrow of a frog in the mercury contained in the smaller branch of the vessel, and place the muscles in the conical part, which is empty: if the aperture be then opened, the mercury, endeavouring to bring itself into a state of equilibrium, will come in contact with the muscles, and contractions will be produced; as an arc of quicksilver will thus be speedily formed between the nerves and the muscles.

VIII. But the experiment may be performed in a manner still more simple. Provide a glass vessel (Plate IV. fig. 10.) filled with mercury, and let the muscles of a prepared frog

be laid to float on the surface of the metal; then suspend the spinal marrow by a silk thread in such a manner, that by letting down the thread the marrow can be made to touch the mercury at pleasure. As soon as the spinal marrow is brought into contact with the surface of the metal, contractions will take place in the muscles; and the same will be observed if a plate of gold or silver be substituted in the room of the mercury. This phænomenon will be exhibited not only by a whole frog, but by half a frog divided longitudinally; which, as soon as it touches the mercury, by the method above described, will be violently convulsed. Having mentioned these experiments to Galvani, to whom I often had recourse for instructions, he regretted that I had confined my researches to frogs only, and advised me to try warm-blooded animals. I therefore took the leg of a lamb or a chicken, and holding it in my hand in a vertical position, in such a manner that the bare muscles were in communication with the mercury, I then raised the crural nerve without any armature, so that, by being left to itself, it could be made to touch the mercury at pleasure. As soon as it did so, I observed a violent agitation and contraction in the whole limb, and likewise when I used the before described apparatus, Plate IV. fig. 2.

IX. While I made these experiments, I was well aware that the contractions produced in the limb might by some be ascribed to the impulse of the mercury, acting on it like a kind of stimulus, or to electricity received from the surrounding

rounding bodies, rather than to the innate electricity of animals. I would advise those who entertain such an opinion to hold the hind legs of a frog in their hand, in a vertical position, and to press only the spinal marrow against the surface of the mercury. Let them immerse also the spinal marrow in salt water, or in vinegar: no contraction will take place, though in this case there is still a mechanical impulse; and though the saline or acid quality of these liquids is exceedingly proper for acting as a stimulus. To this I may add, that in the apparatus already described (Plate IV. fig. 2.) there is no impulse from the mercury, which acquires only that gentle motion necessary to enable it to put itself into a state of equilibrium. In a word, I have observed (Plate IV. fig. 1.) that when both the upper and lower vessels are filled with mercury, if the aperture be opened so that the metal which falls down shall not strike against the muscles; yet the same contractions take place: which indeed ought to serve as a proof that mechanical impulse has no share in producing the effect.

X. But I had no reason to apprehend the action of any stimulus, as I had before found by experience, that a very strong impulse applied to the nerves or to the muscles excited no contractions. I made experiments for this purpose, not on living but on dead animals, when the irritability was feeble and almost extinct; and I found that it could be excited neither by pricking with a needle, by acids, nor by the most powerful stimulants. It seemed to be entirely dead; but I observed

observed both in cold and in warm-blooded animals, provided the experiment was performed within a certain period, that the irritability was always obedient to the power of the Galvanic arc, though no effect was produced by any mechanical impulse. As frogs were most convenient for my experiments, I tried them with every possible kind of mechanical stimulants. I immersed the spinal marrow or the nerves in acids; pierced them with a needle; cut the nerves, and even sometimes scooped out the whole medullary substance from the vertebral canal; still no motion was produced. But the same nerves and muscles which had withstood such powerful mechanical stimulants, when metallic armatures and an arc were applied in the gentlest manner, immediately exhibited contractions.

XI. Having obviated every objection that might be made in regard to the action of stimulants, I shall now endeavour to remove any doubt that may remain of external electricity. Provide a glass cylinder terminating in a neck, and introduce into it a prepared frog with a little mercury; incline the cylinder in such a manner, that the mercury may occupy the lower part of it, and form an armature to the muscles. If the extremity of the neck of the cylinder be applied to an enameller's lamp, and sealed hermetically, all communication between the inclosed frog and the external air will be cut off. Now, if the cylinder be removed from its inclined to a horizontal position, the mercury, which was in contact only with the muscles, must touch also the spinal marrow; and a mer-

curial

curial arc being thus formed, contractions will immediately follow. If the experiment be repeated with the glass cylinder immersed in oil, the same contractions will take place; but in this case it will be necessary to remove the immersed cylinder a little from its position by means of a silk thread, in order that you may be enabled to make the mercury flow from the nerves to the muscles at pleasure.

XII. There is no reason, therefore, in this case, for ascribing the contractions either to the arc or to the armatures, which, as they consist of mercury alone, cannot produce the two kinds of electricity necessary for exciting contractions. But even if we should allow, with those who form the most absurd suppositions, that mercury alone possesses both kinds of electricity, one contraction only could be expected, and not several in succession. In a word, there is no reason to apprehend that any external electricity is obtained either from the glass vessel which receives the mercury, or from the surrounding atmosphere, which is separated from the spinal marrow by three strata of non-conducting bodies; namely, air, glass, and oil. The simplicity of this process may not be fully apparent to the reader; but I can with truth assert, that simplicity was an object which I had always in view. Having prepared a frog, I laid it to float in mercury immersed in oil, and then endeavoured to excite contractions by the application of an insulated arc. Owing to some inaccuracy in the experiment, however, it did not succeed; and my attachment to simplicity, while endeavouring to discover the

least

least complex method of exciting contractions, was the cause of my not obtaining the desired result. This want of success was perhaps owing to the mercury not being in proper contact with the nerves, in consequence of the oil adhering to them. That I might exclude all suspicion of atmospheric air having any share in the phænomenon, I was obliged to adopt that method of performing the experiment which I have already described.

XIII. I however readily foresaw, that the advocates of external metallic electricity might object, that no contractions were obtained but by the application of armatures, or, when armatures were excluded, by using in their stead an arc, which is itself an armature. And I must indeed acknowledge that we are as yet acquainted with no substances but metals capable of exciting animal electricity, though nature, so abundant in resources, may no doubt furnish a great many*. But I shall here observe that metals are not the cause of the contractions produced in animal bodies, but merely a condition requisite for calling forth the latent innate animal electricity which exists in them. For though armatures are necessary to render non-conducting bodies electric, there is no reason why the shock given by the Leyden flask should be ascribed to the arc or to the armatures; as a charged magic square, or a Leyden flask,

* There seems here to be some mistake, as the author says, towards the end of this Dissertation, that he produced contractions in a frog by employing coals, both as an arc and as armature. T.

when

when freed from the armatures, exhibits a great quantity of electricity. If you charge a Leyden flask filled with water, pour the water from it, then pour in other water, and form a communication by an arc between the opposite surfaces, you will experience a shock. This very simple experiment agrees with some made by Mr. Wilson, Mr. Cavallo, and others. Armatures, therefore, have a powerful effect in attracting electricity, and confining it in non-conducting bodies; but they do not supply electricity themselves.

XIV. If this mode of reasoning be admitted in regard to common electricity, it ought not to be rejected when applied to the phænomena of animal electricity. For, as an explosion is produced by a magic square, or Leyden flask, even when the armatures are removed, the case is the same in regard to the contractions in animals. But to prove in a more evident manner that metals have no action in such cases, the following experiment was made. An insulated person, holding in his hand a metallic arc, and a prepared frog, furnished with heterogeneous armatures, was electrified, as well as every thing about him, by means of an electrical machine. The person, the frog, the armatures, and the arc, being all electrified positively, none of the metals employed in this experiment, as their electricity was reduced to a state of equilibrium, could produce contractions. As these are produced only by applying the arc to the armatures, they cannot be ascribed to external electricity, but to the innate electricity of the frog. If only one of the armatures be touched, no contractions take

place; but if the arc be applied to both, they are immediately produced; which is a strong proof that the arc possesses the power of exciting the electricity inherent in animal bodies. Those philosophers would reason very incongruously who should ascribe the cause of these contractions to electricity communicated from the person, since they would thus allow to the person that animal electricity which they deny to the frog; a conclusion which few will admit. But in attempting to remove every suspicion of communicated electricity, it was necessary that the experiments undertaken should be free from all influence of artificial electricity.

XV. I therefore endeavoured to obtain an equilibrium in the armatures by mutual contact. This simple method was borrowed from the principles adopted by philosophers, who, while they endeavour to produce an equilibrium, are accustomed to apply a body electric by excess to one in a contrary state. Immerse in water the spinal marrow of a prepared frog, without armature, and let the muscles rest on a non-conducting body. The vessel must be somewhat in the form of a syphon, and the spinal marrow introduced into the smaller branch must not float on the liquid, but be totally immersed in it: this arrangement is of great importance in regard to the success of the experiment: a small piece of tin foil must be made to float on the surface of the water at a considerable distance from the spinal marrow: if you then touch the muscles with one of your hands moistened a little with water, and with the other apply a silver arc to the tin foil,

foil, a contraction will immediately take place. While struck with the constancy of this phænomenon, I could not help reasoning in the following manner: Muscular motion is produced though the frog is in contact with no metallic body: every thing metallic is separated from the frog; and even if it possessed contrary kinds of electricity, they are in a state of equilibrium. What then is the external agent which produces contractions in the frog? Though the metals, while they acquire an equilibrium, come into contact, which is followed by contractions, there certainly can be no fear of external electricity from them. This is sufficiently proved by the metals themselves being brought into equilibrium before the contractions take place. If you touch the muscles with one hand moistened with water, and with the other immerse into water a piece of gold coin, a small part of which is covered with tin foil, contractions will immediately ensue. Yet both the metals, before the contraction, were brought to a state of equilibrium by being in contact: they can therefore have no share in the contractions, which must arise from innate electricity. If salt water, milk, serum, or the crassamentum of the blood, be substituted for common water, there will still be contractions; and the case will be the same if a bit of gold, silver, or brass, or even iron, covered with tin foil be employed. Here then we have tin brought into a state of equilibrium with various metals before the contractions take place, without these contractions being impeded. Nor is it of any consequence that the metals be touched with the

the hand. For, if the tin foil be applied to the tip of the tongue, or to the lips, and if a piece of silver wire be made to touch the tin foil on the one side, and the spinal marrow covered by no metal on the other, contractions will be produced as often as an arc is made to the muscles by means of the moistened hand.

XVI. The object of our researches hitherto has been muscular motion; we shall now direct them to the phænomena of the senses. Let an insulated person be electrified by means of a common electrical machine, and let him apply a silver arc to the tip of his tongue covered with tin foil. The armature and the arc will both become electrified by this new accession of electric matter. We cannot therefore suppose one armature to be positively and the other negatively so; and for this reason no transmission of electricity, and no sensation of taste, can be expected. As the tongue, however, experiences a sensation of acidity, it must have been excited not by the armatures or by the arc, but by the innate electricity of the muscles and nerves. But the necessity of an internal arc, which appears in the above phænomena, is an argument in favour of innate electricity. In order, however, to excite the taste by animal electricity, the application of different metals to the tongue is not sufficient: the arc must be conveyed to the muscles of the tongue, or to others at a distance from it. When I first made this experiment, it gave me no small pleasure to find that it was confirmed

and

and enriched with new observations by the celebrated Volta*.

XVII. That the phænomena already mentioned arise from an interior arc, will be proved, in my opinion, by the following experiment: If an insulated metallic arc, or a piece of charcoal, be applied to the tip of the tongue with the moistened hand, no taste will be produced; but if the same hand be applied to the same metallic arc or charcoal, a taste will be experienced. In both cases of this experiment dissimilar armatures were brought into contact, and reduced to that state of equilibrium to which they tended. Why then should the re-

* The celebrated Volta, in a letter which I received from him, announcing that he had observed the same phænomena as those described in my Dissertation, published the preceding year, § xxii. p. 19. added the following remarks: "The best and easiest method of performing this experiment, is to immerse in a large earthen or glass vessel, filled with water, a silver dish, in such a manner that a part of it remains above the surface of the water; to apply to the tip of the tongue a small bit of tin foil, so that part of it shall hang out of the mouth; to bring this tin foil into contact with the silver vessel, either immediately or by means of a third piece of metal; lastly, to immerse the hand in the same water gradually, if you are desirous of perceiving gradually on the tongue the acid taste; or suddenly, and at once, if you are desirous of perceiving it at once and in the highest degree. A silver spoon half immersed in the water, or instead of the dish, if not too small, will produce nearly the same effect as I have already mentioned. The case is not the same with a slender silver rod or wire, which if gradually immersed will scarcely produce any taste at all. If the vessel which contains the water be itself of silver, a dish or spoon will then be unnecessary. This vessel forms the best armature for the water; and to perceive the taste very strongly, it will be sufficient to immerse the hand in the water, or to bring the tin foil which hangs from the mouth into contact with the vessel.

sult

sult of the experiment be so different? When the metals or charcoal touch the moistened hand, a speedy and uninterrupted communication is formed even between the remote muscles and the nerves of the tongue, which when intercepted by the non-conducting body prevents all sensation of taste. It may therefore be established as a principle, that to excite a sensation of taste, it is necessary besides the application of external armatures to have an internal arc, which may bring the internal electricity to a state of equilibrium. This observation is confirmed by Carradori, who, while endeavouring to excite a sensation of taste in two persons at the same time, found that it was necessary to establish an arc between them, either by making them join hands, or by moistening the plane on which they stood*.

XVIII. I shall conclude this dissertation with an account of some later experiments on this subject. Volta, in a letter which I received from him, requested I would try to produce contractions without any metallic application, and recom-

* As convulsions are excited in two frogs, when one end of an arc is made to touch the uncovered crural nerves of the one, while the other end of the arc is applied to the crural nerves of the other, covered by an armature; I have observed that the sensation of two tastes, one acid and the other alkaline, can be excited at the same time in the tongues of two persons, one of which is armed with tin foil, and the other with silver, if a communication be formed between the two armatures. It is necessary, however, that a communication alſo ſhould exist between the two persons. If the floor on which they stand be wet, and their shoes moist, this will be sufficient. *Sig. Dottore Giovachino Carradori Lettera quinta sull Ellettricità Animale, diretta al Chiarissimo Sig. Cav. Felice Fontana.*

mended

mended charcoal, which in his first experiments he had found to be the best armature for animal electricity. I therefore took the earliest opportunity of attempting to produce contractions in the beforementioned manner without the aid of metals. I was encouraged in this design by Aloysius Laghi, professor of chemistry; who having analysed our fossil coal in consequence of a public decree for that purpose, was desirous that chemical processes might be made subservient to my researches. It was well known, that every kind of vegetable charcoal formed the best armature, so that when this coal was used there was no need for metallic armatures. Hence, in that Galvanic experiment called the animal alarum, a charcoal plane substituted for one of silver produced the same effect: charcoal arcs also were used instead of metallic. The coal employed in this manner was vegetable coal: but the English fossil coal, and that dug up in our territories, did not produce the same effect. I employed the different principles extracted from our fossil coal, namely, calcareous earth saturated with the acetous acid, siliceous earth semi-vitrified by fixed alkali, and argillaceous earth. All these, however, formed bad armatures for animal electricity; and the case was the same with the ashes of our fossil coal, and of the English coal.

XIX. None of these phænomena, however, afforded any grounds for objecting against the theory of electricity in general; as that bituminous substance which is always found combined with fossil coal, deprives it of the power of being a conductor

ductor of animal electricity. This conjecture was confirmed by experience; for, having employed our own fossil coal and the English in the state of coke, they formed excellent armatures, as by the action of the fire they had been freed from those idio-electric principles which opposed the development of animal electricity. A phænomenon in the mean time occurred, which tended to throw great light on the nature of this electricity. Having placed the spinal marrow upon a piece of coke, and formed an arc from the muscles to the coke, contractions always took place in certain parts, while in others there was no appearance of them. The reason of this seemed to be, that the action of the fire had made some parts of the same coal conductors, and left others idio-electric, in consequence of the large quantity of the bituminous principle which they contained. But though torrified fossil coal acquired a conducting property, vegetable charcoal was still found to be much fitter for conveying animal electricity. Hence I conceived a hope, that I should be able to excite contractions, in the manner before described, without any metallic arc, and by the application of charcoal alone. For these new experiments, I employed the largest frogs, and I selected on purpose such pieces of charcoal as seemed the least fitted for being conductors of animal electricity. I placed the prepared muscles of a frog on the charcoal, and suspended the unarmed spinal marrow, by a silk thread, in such a manner that the marrow could be made to touch the charcoal at pleasure. When large frogs were employed, contractions always took place; and Galvani found the case to be same in

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his experiments. Here then we have contractions produced without the intervention of any metallic substances: why then ascribe to the different power of metals, effects which can be produced by bodies which certainly have nothing of the metallic quality? If the spinal marrow or muscles be made to communicate separately with the charcoal, there will be no contraction; and it appears that to produce contractions, the arc and the armature must consist of homogeneous charcoal. Having given an account of my experiments, it remains that I should collect in a few words the inferences which may be deduced from them.

XX. In the first place, it is certain that to produce contractions it is not necessary to employ two different kinds of metal, and that one is sufficient. In vigorous animals this result may be obtained by silver, and particularly by gold.

2d. If any suspicion of heterogeneity should arise, in regard to the solid metals, this difficulty may be easily obviated by employing a fluid metal, that is to say, mercury purified by chemical means.

3d. Contractions are excited when one of the armatures and the arc consist of mercury, by making the mercury to run down on the muscles placed below it. There is no reason here to suspect that the stimulus produced by the impulse of the mercury has any share in the phænomenon, as it is sufficiently proved by experiments that this is not the case.

4th. That when there is no reason to suppose a want of equilibrium in the electricity of the armatures and of the arc, animal electricity is excited, and produces contractions.

5th. That when the armatures and arc are formed of charcoal, the same results will be obtained; which evidently proves that the animal electricity is not produced by the metals.

SECOND

SECOND DISSERTATION

ON

ANIMAL ELECTRICITY:

READ IN THE

INSTITUTE OF BOLOGNA,

IN THE YEAR 1794,

BY J. ALDINI.

I. THE philosophers of the present period are so sanguine in their expectations, that when a new theory is proposed, unless it be presented to them perfect and fully proved, they either attack it in part, or entirely reject it. Such has been the case with animal electricity, discovered by Galvani. It is urged against it by its opponents, that it is subject to variations; and because they do not find it obedient to all those laws established by the laborious researches of a Franklin, a Beccaria and an Æpinus, they assert either that it has no foundation, or that it is contrary to nature. It has therefore

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been conceived that an accurate comparison of animal and common electricity, in order to ascertain whether there be any difference between them, might be the best means of obviating such objections. For this purpose I made various experiments in animal electricity under the air pump, employing proper conductors, and I compared its phænomena with those exhibited by the Leyden flask. In a word, while I endeavoured to pursue my researches agreeably to the general theory of common electricity, my principal object was to prove the constancy of animal electricity, by discovering, if possible, an agreement in the physical laws of both.

II. As it had been proved by a great many experiments, that common electricity could be obtained from non-conducting bodies in the most perfect vacuum possible to be formed by means of an air pump, experiments were undertaken in order to ascertain whether the same phænomenon was common also to animal electricity, and with this view attempts were made to excite the latter in vacuo*. Muscati indeed had deprived animals of life in vacuo, and afterwards found them susceptible of Galvanism in the open air; but he made no attempt to determine whether the animal electricity could be manifested in vacuo. I employed for my experiments a glass vessel, Plate IV. fig. 6, furnished with a metallic rod, which could be raised up or pushed down at pleasure. To the ex-

* As this term is improperly used by philosophers, I must here observe, that I shall in future understand by it air highly rarefied by the usual means.

tremity

tremity of the rod, within the receiver, was affixed at right angles a metallic wire, from one end of which an armed frog was suspended by the muscles, and from the other a small metallic chain a little longer than the frog, a plate of silver being placed below both the frog and the chain. When as perfect a vacuum as possible had been obtained, the metallic rod was pushed down, so that the small chain and the spinal marrow of the suspended frog were made to touch the silver plate, and by these means the latter formed an arc in the exhausted receiver. In this experiment, the power of Galvanism was found to be the same as in the open air, so that as often as the rod was pushed down contractions were excited in the frog. By this method it was easy to ascertain what repeated contact could produce by forming new arcs: for though the small chain and the extremity of the spinal marrow touched the silver plate; yet, when removed from that position ever so little, by moving the rod new contractions took place, which could not have been expected unless new contact on moving the rod had produced as it were new arcs. This kind of apparatus seemed the most convenient for performing in vacuo all those experiments which Galvani had performed in the open air.

III. But it was as yet difficult to determine, whether the contractions which took place were stronger in rarefied than in common air; for the difference between the electricity was so small, that it was impossible to say which was the more powerful. I therefore resolved to clear up this point by other experi-

experiments. For this purpose, having cut in two a prepared frog, I placed in vacuo one part of it, by means of the above apparatus, and after a short period drew it out, and compared it with the half which had not been subjected to the action of a vacuum. On applying an arc, the one exhibited strong and the other faint contractions; from which it appeared that the vacuum had occasioned some loss of the animal electricity; as the muscular and nervous parts subjected to experiment belonged to the same frog, this evidently showed that the whole difference arose from the action of the vacuum.

IV. It is well known that a vacuum absorbs common electricity; and therefore it need not excite any wonder that in the present experiment it should have dispersed some of the animal electricity. As this loss took place by insensible degrees, strong contractions were not to be expected; and the case is nearly the same in a Leyden flask loaded with aqueous vapours, which produces no remarkable explosion. But though convinced of the truth of this circumstance, I resolved to confirm it by a new experiment. I therefore charged two Leyden flasks armed in the same manner, applying them at the same time to the same conductor, and by the same number of turns of the machine. One of these flasks was introduced into the glass receiver of the air-pump, and the other was exposed to the atmosphere. At the end of five minutes after the air had been exhausted as much as possible, the flask in the receiver was taken out, and, being discharged by a metallic arc, emitted a weak spark, while the other flask exhibited

strong

strong signs of electricity I again charged two flasks by the same turns of the machine till the electrometer indicated in both the same degree of electricity, and kept one of them for half an hour beneath the receiver. When the latter was taken out, it afforded a weak and almost exhausted spark; while the other, which had remained in the open air, emitted a strong one. Had the first flask remained longer under the receiver, it would no doubt have entirely lost its electricity; while the other without the machine retained that with which it was charged.

V. In this experiment every possible care was taken to observe the variations produced by the vacuum. The receiver which covered the Leyden flask was of a large size, and the flask, being placed in the middle of it, was at a considerable distance from its sides: the receiver was well fitted to the plate of the machine, and not by means of moist leather, as is usual, so that all communication with the external air was prevented; and therefore there is no reason to suppose that any aqueous vapour introduced into the machine, when the pressure of the air was withdrawn, could perform the office of an arc. That the electricity might not immediately bring itself to a state of equilibrium, the conductor of each flask terminated in a ball: had not this been the case, the whole electricity would soon have been dispersed; for, when the conductors terminate in a point, if the chamber be darkened, coruscations of light will every where be seen on the glass receiver, and afford a most agreeable spectacle. A conductor terminating in a ball

ball seemed therefore to be most convenient for my purpose, that, by rendering it more difficult for the electricity to bring itself to an equilibrium, I might be able to produce a greater imitation of the intimate manner in which animal electricity adheres to the animals. But though in the above experiments I ascribe some part of the phænomena to the animal electricity being weakened in vacuo, I am of opinion that more is to be ascribed to the violent perturbation of the principles which the vacuum must have excited in the muscular and nervous parts.

VI. As it was now established, that animal electricity could be excited in vacuo, I endeavoured to ascertain whether that excited without the vacuum, and conveyed to the receiver, could be made to pass through a very small space in vacuo. For this purpose I placed the metallic rod at a small distance from a silver plate resting on the bottom of the air-pump: the limbs of a chicken or lamb, prepared in the usual manner, were then deposited near it; and the muscles, by means of a metallic chain, were made to communicate with the plate of the machine, while an armed nerve was made to communicate by means of an insulated arc with the metallic rod. The air being exhausted, the rod was pushed down, and gradually brought as near as possible to the plate without coming into actual contact with it. In this state no contractions were produced; but they immediately took place when the rod was brought into contact with the plate. It appears therefore that animal electricity is considerably impeded

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in its progress by a vacuum; and that, like artificial electricity, it does not readily suffer itself to be dissipated, unless transmitted through good conducting bodies. For, when a small quantity of the electric fluid is accumulated in the Leyden flask, either none of it proceeds from the metallic wire to a less perfect conductor, or, if any is transmitted, it must be with difficulty, and with great violence. Common electricity, indeed, is seen to pass quietly through metals or water separately; but a strong electric spark, in proceeding from one metallic conductor to another, if it pass through water interposed between them, does so with such violence, that the glass vessel which contains the water is in danger of being broken to pieces. That electricity, therefore, may be conveyed from a conducting body, to one less endowed with that property, it must be in such abundance as to be able to overcome the resistance of the body through which it has to pass: hence, it need excite no surprise, that the small quantity of electricity which produces contractions should not be able to pass over a very small space in vacuo. Here then we have a proof that animal electricity is not only subject to the laws of non-conducting bodies, but that it is affected different ways by the smallest obstacles of conducting bodies, and by different kinds of them. It is indeed so evident that a vacuum from its nature is unfit for conveying electricity, that, even if one be produced, not by the usual method, which is always attended with some defects, but in the most accurate manner possible, it is totally improper for being a conductor

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of the electric fluid. This is sufficiently proved by the following experiment of Walsh: If two barometers be joined, and the upper part or bend be carefully deprived of air, when one of these barometers is electrified, the electric fluid will not be communicated to the other, in consequence of the resistance opposed by the intervening vacuum. Adams, however, exhibited by means of a single barometer the same phænomenon as Walsh did with a double one*; for, having extracted the air entirely from the upper part of the barometer, no electric light was observed; but on introducing a very small quantity of air the whole barometer became luminous. These observations are sufficient to show that animal electricity, in regard to the property of not being able to pass through a small space in vacuo, is subservient to the general laws of common electricity. But let us proceed to other phænomena respecting animal electricity excited in vacuo.

VII. If a prepared frog, furnished with two armatures, be

* In the description of a new air-pump of his invention, where he ſhows that electricity cannot pass through a vacuum, he adds: "There can be little doubt, from the above experiment, of the non-conducting power of a perfect vacuum; and this fact is still more strongly confirmed by the phænomena, which appear upon the admission of a very minute particle of air into the inside of the gauge. In this caſe, the whole becomes immediately luminous upon the slightest application of electricity; and a charge takes place, which continues to grow more and more powerful, in proportion as fresh air is admitted, till the density of the conducting medium arrives at its maximum. *An Essay on Electricity, explaining the Theory and Practice of that useful Science.* Third edit. London, 1787.

be placed in a horizontal position on a non-conducting body, under a glass receiver, (Plate IV. fig. 11.) and if an arc be formed by pushing down a rod, so as to join both the armatures without touching the frog, contractions will immediately take place; but if the smallest non-conducting body intervene, none will be produced. I found it no very difficult matter to exhibit in vacuo, by an apparatus somewhat similar, what may be called an animal alarum. A horizontal arm fixed to a vertical moveable rod (Plate IV. fig. 5.) was adapted to the inside of a glass receiver, in such a manner that, when the receiver was exhausted, any body resting on the horizontal arm could be made to fall down. The leg of a prepared frog was then fixed to the vertical rod; while the other rested on the horizontal arm, and the spinal marrow, with an armature of tin foil, touched a silver plate in the bottom of the receiver. The horizontal arm being turned round a little, by means of the vertical rod, the leg of the frog resting on the arm fell down on the plate below: an arc being thus formed, contractions immediately took place, and were incessantly repeated, until all the animal electricity was restored to an equilibrium.

VIII. These experiments were made on dead frogs; but I shall here show that living ones also may be made to exhibit signs of electricity under the same circumstances. A piece of tin foil was applied to the back of a frog tied to a silver plate by means of silk strings, (Plate IV. fig. 4.) and two metallic chains were suspended in such a manner, that by pushing

 down

down the rod the extremity of the one chain could be made to touch the silver plate, and the extremity of the other the tin armature: when an arc was by these means formed, contractions instantly took place. The frog began to breathe with difficulty, to be agitated with convulsive movements, and to be seized with an universal tremor, so that its last moment seemed to be approaching; but on air being admitted into the receiver, it recovered and appeared as lively and active as before. On examining more closely the changes which had taken place in the animal while in this state, which was certainly contrary to nature, I found the muscles red with a superabundance of blood: but when dissected in the usual manner, they exhibited strong signs of animal electricity; for, on applying an arc to the nerves or muscles, without any armature, violent contractions were produced, and continued for a long time, provided care was taken that there should be no deficiency of animal moisture. That such a quantity of electricity should be excited, will not seem astonishing to those who have seen more violent electric commotions excited in the animal machine by the action of a needle. This indeed has been placed beyond all doubt by an observation of Gardini, who says "that having made some experiments with a large torpedo, he remarked that stronger convulsions were produced when the animal was subjected to great pain by any means, such as pricking it with a needle*." The phænomena

* Josephi Gardini de Electrici Ignis Natura Dissertatio, Regiæ Scientiarum Academiæ Mantuanæ exhibita, Mantuæ 1792, p. 100.

exhibited

exhibited by a vacuum or rarefied air, were exhibited also by condensed air, so that very powerful contractions were produced by one homogeneous arc. For, if the same apparatus described in Plate IV. fig. 4. be adapted to a condensing machine, a dead frog introduced into it will readily be contracted. Live frogs also, after being kept for half an hour or a whole hour in air twice as dense as that of the atmosphere, exhibited strong signs of electricity without any armature, and merely by the application of a silver arc to the nerves and muscles.

IX. Having made these experiments in vacuo and condensed air, it was of some importance to try also what effect would be produced on animal electricity by the action of the aëriform fluids. The apparatus employed for this purpose was as follows: I provided a glass vessel (Plate IV. fig. 3.), terminating at the upper extremity in a neck, to which could be closely fitted, when necessary, a metallic cover, having a perforation in the centre to receive a moveable rod, which was connected with a transverse metal conductor supporting a frog prepared in the usual manner. Having filled the vessel with water, or mercury, which was still better, I placed it on the shelf of a pneumatic tub, and introduced into it, according to Priestley's method, any particular gas. Some water or mercury was put into the dish to which it was afterwards removed; and the metal cover, having a frog suspended from it, being then fitted to the neck of the vessel, the frog by means of this apparatus could be immersed

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at pleasure in carbonic acid gas, hydrogen, or any other kind of gas; so that the constancy of animal electricity might be tried in either. It is evident that, by letting down the spinal marrow of the frog, and the end of the conductor to the surface of the water or mercury, by means of the rod, an arc will be formed, and that muscular contractions must then be produced. The same experiment may be performed by means of a bottle (Plate IV. fig. 6.) furnished with a cover like the former. After being filled with water or mercury, it is inverted on the shelf of the pneumatic tub, and the gas is then introduced in the usual manner; but care must be taken not to displace the whole of the water or mercury, as a portion must be left to cover the bottom when the jar is turned up. This experiment I tried only with oxygen gas, reserving the other kinds of air till a more convenient opportunity.

X. To give more weight to these experiments in vacuo, I endeavoured to prevent all those errors which arose, or might be suspected to arise, from the introduction of air; for I supposed that some opponent of the theory of animal electricity, while endeavouring to find out objections against it, since there was no foundation for asserting that the vacuum afforded any electricity to the animals, might pretend that, the plate of the pneumatic machine being metal, according to custom, some electricity from the atmosphere might be attracted by it, and be thus conveyed even through a vacuum to the animals. On this account it was necessary that a

vacuum

vacuum should be formed, without placing the receiver on a metallic plate; without the contact of any conducting bodies, and in such a manner as to show that the electricity excited was that really existing in the animals. I therefore employed a glass receiver (Plate IV. fig. 9.) cut into two parts above its middle, between which was placed a horizontal partition, in order that the upper part might be filled with oil, or some non-conducting body. The partition was perforated with a large aperture, the superior edges of which projected a little upwards; and the lower, projecting downwards, were furnished with small circular grooves, so that the hole could be shut by a piece of bladder tied over it with thread. A sharp-pointed rod was placed over the hole, so that, being let down by a non-conducting handle, it could be made to pierce the bladder. This, however, was to be done only when the lower part of the receiver was exhausted of air; for the oil, when the air is withdrawn, speedily falls down: by these means a vacuum is formed in the upper part, and, in consequence of the interposed stratum of the oil, remains insulated, as it can receive no electricity from the metallic plate of the machine, nor from other conducting bodies. But it will be in vain to attempt to form a vacuum in this manner, unless the parts of the cut glass be so fitted as to prevent entirely the admission of external air. A very simple and ingenious apparatus, for the same purpose, was invented by F. Borelli: in order to stop the efflux of the oil at pleasure, he adapted to the hole a glass cone, by which means he was enabled to produce an insulated vacuum of greater or less

capacity in the upper part of the receiver. Here then I had a vacuum every where surrounded with non-conducting bodies, so that, if animal electricity were excited in it, there could be no reason for ascribing it to electricity borrowed from the atmosphere.

XI. I adapted to a glass rod, in a longitudinal direction, a metallic plate, (Plate IV. fig. 7.) which at the upper extremity was bent into a right angle, and supported the spinal marrow of a frog, so that it might be considered as a lengthened conductor of the nerves. This plate was inserted into a glass receiver, so as to move in it with ease; and by means of a screw could be fixed at any altitude whatever. An accurate representation of this apparatus, with the glass rod annexed, is seen in Plate IV. fig. 8. Iron hooks fixed to the feet of the suspended frog acted the part of a conductor to the muscles; and the upper part of the receiver being exhausted of air, as above described, a stratum of oil was still left to separate the frog from the partition. To guard against all danger from the action of any internal metallic body, a strong magnet was applied to the iron conductor of the muscles, which, immediately obeying the power of attraction, fell upon the conductor of the nerves; and thus a circuit of animal electricity from the muscles to the nerves being speedily effected*, contractions were immediately produced. When I com-

* This may be accomplished in a much more simple manner, without the aid of a magnet, by connecting a wire with the lower part of the nerves, and applying the wire to the muscles by turning the rod round.

municated

municated this experiment to the Institute, I was extremely desirous that the apparatus I had here invented for the purposes of animal electricity might be of some advantage to the science of philosophy in general, of which I was always fond, and which formed the chief object of my study. But it is necessary that I should mention to what I more particularly allude.

XII. Such bodies as had hitherto been put into the air pump were gradually subjected to the action of a vacuum. Hence it appeared that the apparatus above described might be attended with a considerable degree of utility; as in future, any body whatever, whether solid or fluid, might be subjected to this action at once. For, the lower part of the receiver being deprived of its internal air, if the bladder be burst by means of the glass rod, the fluid will run down and occupy the space emptied of air, leaving a vacuum in the upper part. When the fluid has thus fallen to the lower part of the receiver, solid bodies even, if any were immersed in it while in the upper part of the receiver, will also experience the action of the vacuum. But liquid bodies are of such a nature, that they have united with them certain aëriform fluids, which, when the pressure of the air is removed, readily expand. A fluid, therefore, when it has fallen to the lower part of the receiver, being agitated and thrown into a state of perturbation by the motion, its most subtle principles will be extricated, and fill the capacity of the bell. The vacuum will then be disturbed by the evapo-

ration, which, acting on the mercury in the barometer connected with the air pump, will cause it to fall. But every one acquainted with the principles of philosophy must know, that the depression of the mercury in the barometer will be greater, according as a greater quantity of aëriform fluids has been disengaged in the receiver; and if the degree of pressure in the barometer varies according to the variety of aëriform principles, it may be readily seen, that this method may be employed to determine the quantity of them, or their elasticity, since they are cut off from all communication with the surrounding atmosphere, though it still exercises its pressure upon them.

XIII. The vacuum here obtained in the upper part of the bell, seems to be far superior to that produced in the lower part, according to the usual method. For it has long been a complaint among philosophers, that by working the air pump the air is only rarefied, till it no longer possesses elasticity capable of raising the valve, so that it is impossible to produce a complete vacuum by this method. If we can credit the followers of Euler, that subtle fluid, which they call ether, and which permeates every thing, still remains; for, adopting the opinion of the antient Peripatetics, they consider a vacuum as beneath the dignity of nature. But, setting aside this question, I shall only observe, that if a vacuum be formed in the upper part of the receiver, by the method above described, it does not appear that it can be disturbed by any thing from without, and the gravity of the falling fluid

fluid will not suffer itself to be overcome by the subtle ether, if any really exists. Should it be apprehended that the air contained in the oil may be disengaged, and disturb the vacuum, you may substitute in its stead mercury or water, which can be deprived of air either by boiling or by long exposure to the action of a vacuum. But before any thing certain on this subject can be said, new and repeated experiments will be necessary. As every objection that could be made in regard to a vacuum seems now to be obviated, since a space perfectly free from common air can be produced, and cut off from all communication with the atmosphere, or with conducting bodies, I shall return to animal electricity, from which I was led by a desire of contributing towards the improvement of natural philosophy in general.

XIV. Those who attempt to determine the velocity of the nervous fluid in a given time, undertake a matter of great difficulty, respecting which nothing certain can be known. Haller rejected the suppositions of those who, comparing the tenuity of the nervous tubes of the heart with the large branch of the aorta, were of opinion, that the velocity of the nervous fluid must be two thousand eight hundred and eighty times greater than that of the blood. This celebrated physician, distrusting hypothesis, had recourse to experiment, and found that the velocity of the nervous fluid would be no less than nine thousand feet in the first minute. But in this determination of the velocity of the nervous fluid

there seems to be some difficulty, which perhaps ought to be ascribed rather to the period when that celebrated man lived, than to his want of sagacity or accuracy. Had Haller possessed the means of conveying the nervous fluid with his own hands to different parts at pleasure, he would no doubt have given us some more certain ideas respecting its rapid motion. I resolved, therefore, not to neglect those advantages with which the modern philosophy has been enriched, and to employ very long metallic arcs, by which I could direct the animal electricity as I pleased. A staircase which reached from the top to the bottom of the house, with many windings, presented me with an iron plate, exceedingly convenient for the transmission of animal electricity. A metal wire, brought down from the top of the staircase, was connected with the iron plate, and by these means I obtained an arc, the length of which was above one hundred and fifty feet. When this arrangement was made, the two extremities of this very long arc were applied to the armed nerves and muscles of a frog; and the animal electricity being thus excited, instantly proceeded with so much velocity from the one extremity of the arc to the other, that no difference could be perceived between the time when the frog touched the arc, and that when it began to be agitated. But to show that this result was not owing entirely to the metallic conductor, I employed long ropes dipped in salt water, and always with the same effect. This circumstance seems to prove, in a striking manner, a great similarity between the nervous fluid and common electricity, and to overturn the

opinion

opinion of Haller, who, according to his calculations, makes the nervous fluid require a second for passing over the space of 150 feet.

XV. According to Beccaria, a celebrated observer of the propagation of the electric fluid, there are two ways of its being transmitted; one when it flows through conducting bodies only, and the other when, being collected in non-conducting bodies, it proceeds from the coating electric by excess to that which is in the contrary state. In the one case, Beccaria establishes a certain time for its passage; in the other he allows none. This celebrated man observed, that the electric matter was conveyed from the conductor of a machine, along a metallic wire 500 feet in length, in the course of a second; in its passage along a hempen rope of the same length, it employed seven seconds; but when the rope was moistened with water, it required only two or nearly three vibrations of a pendulum. When he discharged a Leyden flask by the longest conductor, he was never able to observe the least interval of time. The same thing was remarked by Jallabert, Sigaud de la Fond, and other philosophers, who performed their experiments, not in an apartment but in the open air; and conveyed the electric fluid in this manner for a considerable distance along the banks of large rivers. Monnier extended two iron wires* in an open field, parallel to each other, for the distance of 5107 feet; and a man placed between them held in

* Précis historique et expérimental des Phénomènes électriques, par M. Sigaud de la Fond, Paris 1781, sect. i. art. 4.

his hands the extremity of the conductors, keeping them at a little distance from his body. "But the man, who was in the middle of the arc," says the author, "while he saw the spark issue from the jar, received the shock: he could have distinguished the smallest interval of time between the explosion and the shock; and if it had amounted to the fourth part of a second, it could have easily been remarked."

XVI. While reflecting on these facts, I formed a conjecture from the great celerity with which animal electricity is conveyed, respecting the manner in which it is evolved. If the animal electricity were conveyed from a muscle to a nerve, or vice versa, in the same manner as the common electric fluid is conveyed from the machine by the chain, it would have been observed to employ some time, however small, in its passage. As I at first ascribed this to the shortness of the conductor I had used, I extended it to more than 250 Parisian feet, and applied the nerves and muscles of a frog to this new conductor in the manner above described, without observing the least obstacle to the passage of the electric fluid. As this arc formed a half of that employed by Beccaria, the space of half a second would have been required, if we consider in this passage only one kind of electricity. But the half second required according to the observations of Beccaria was not observed: it therefore appears, that this propagation of animal electricity ought not to be referred to the first-mentioned case, but to that where equilibrium is restored between the negative and positive state.

XVII. This rapid conveyance of animal electricity, however, is entirely stopped, if the metallic arc be intercepted, not only by non-conducting but by certain conducting bodies. Here then we have again occasion for the action of two contrary kinds of electricity. For the electric matter, whether positive or negative, when conveyed from the machine to the chain, pervades all bodies in the same manner, provided they be conductors. Thus the metallic conductor of the machine, the insulated person, and all other bodies that may be connected with it, become electric in the same manner. But the same electric matter collected in non-conducting bodies requires, before it can be discharged, certain conditions in the conducting bodies by which it is discharged. When the metallic arc is interrupted by a little water, the Leyden flask, if it contain a moderate quantity of electricity, cannot be discharged; for the two contrary kinds of electricity to be discharged require that every part of the arc should be equally endowed with the property necessary for conducting the electric fluid. But in the case of only one kind of electricity, it would pass with great readiness either through water or metal. Let us now apply these phænomena of general electricity to the theory of animal electricity. In the experiment mentioned in the sixth section, if only one kind of electricity proceeded from the nerves or the muscles, it would be immediately conveyed from the nerve to the metal, then to the vacuum back to the metal, and thence to the muscle, as being the place from which it issued. Besides, the electricity propagated in this manner would have produced contractions, which

which however were not observed. The progress of the animal electricity, therefore, experienced considerable obstacles, not only from non-conducting but also from conducting bodies; which affords a strong proof that electricity exists and is collected in the muscular fibre, in the same manner as in non-conducting bodies. But the remarkable quickness of the progress of animal electricity leads me to the Leyden flask; and therefore I shall here say a few words respecting the analogy between the phænomena it exhibits, and those of animal electricity; and give an account of the reasons which first induced me to enter on this comparison.

XVIII. When public meetings were held at the house of Galvani, for the purpose of discussing the theory of animal electricity, great doubts were entertained respecting two contrary kinds of electricity acting in animals. The reasoning on this subject, which displays acuteness and ingenuity, was as follows: If two kinds of electricity, one positive and the other negative, prevailed in the nerves and muscles of animals, on applying the muscles of one frog to the armed nerve of another, we should observe contractions; which however is not found to be the case. The proposed doubt, however, gave me considerable uneasiness, as the dispute on that subject seemed to lay a foundation for many objections against the theory of animal electricity. But the novelty of this event will excite no astonishment in those who consider the subject with attention: nay, it would rather seem astonishing if the matter were otherwise. The phænomena in the above experiment

riment are perfectly agreeable to the laws of general electricity, and to the theory of the new animal Leyden flask. For, if we suppose the muscles of the frog furnished with nerves to represent so many Leyden flasks, no contractions ought to be expected from them, in circumstances under which Leyden flasks themselves would produce no explosion. This we always observe in two electric jars, neither of which is discharged when the arc is established between the exterior coating of the one and the interior coating of the other. If the muscles, therefore, in the above experiment, represent Leyden flasks, in cases in which no explosion could take place in the latter no contractions can be observed in the former.

XIX. For the sake of illustrating the proposed analogy, it will be proper that I should here explain the conditions under which contractions are produced by the application of two frogs to each other, and compare them with the phænomena of the Leyden flasks. I shall therefore show, in a few words, the different methods in which several frogs are made to contract at the same time, and in which Leyden jars are discharged. Place on a glass plate two frogs, one of which has its spinal marrow armed, and let a communication be established between its muscles and the spinal marrow of another frog, by means of a small metallic chain. If the arc be formed from the armed spinal marrow of the one frog to the muscles of the other, contractions will be produced in both. Let us now apply to Leyden flasks the arrangement followed with the frogs. If two electric Leyden jars stand on a glass plate,

 when

when an arc is applied, some electricity will be elicited; but an absolute explosion will never be produced. If one extremity of a metallic wire, however, be brought in contact with the inside of one of the Leyden jars, and the other with the exterior coating of the other, on applying an arc to the other two coatings, which have no communication with the metallic wire that has been added, an explosion will take place, and both the jars will be discharged. But the above contractions may be produced in a manner still simpler. If the spinal marrow of one frog be united to the muscles of another, as soon as an arc is formed from the spinal marrow of the armed frog to the remote muscles of the other, strong contractions will be produced in both. The analogy between animal and artificial electricity, which is the object of our research, will always be apparent in this experiment. Two charged Leyden flasks, suspended in such a manner from the conductor of the machine that the exterior coating of the one is connected with the interior coating of the other, form a very happy representation of the frogs; for, the same arc being applied, and in the same manner, both to the frogs and the Leyden flasks, when an explosion is produced by the latter contractions will take place in the former.

XX. Hitherto the contractions have been produced by establishing the arc from the nerve of one frog to the muscles of another: but contractions will take place in both, if the arc be conveyed from the armed spinal marrow of the one to the armed spinal marrow of the other, provided care be taken that

corresponding

corresponding muscles communicate alternately with the conducting body. But it is much more difficult to reconcile this phænomenon than the former to the general laws of electricity. This difficulty, however, may be obviated, if we suppose that the one frog, in consequence of its natural moisture, forms an arc to the other. This indeed was first confirmed by the experiments of Galvani; for, having divided a frog lengthwise, both parts were connected merely by their moisture*. Yet, when the arc touched one of the separated parts, the other was immediately contracted. As this explanation is so obvious, nothing further needs be said on the subject. But I was unwilling to leave in a state of uncertainty the analogy between the phænomena of animal electricity and those of the Leyden flask, the wonderful agreement of which had so much excited my astonishment: and indeed I had no cause to repent of my perseverance; for, though it did not enable me to attain to what I proposed, it conducted me to some general phænomena of electricity, which no one perhaps had before made an object of research. I discovered that one Leyden flask may be applied as an arc to another. I provided two insulated Leyden jars of the same capacity, one of them charged and the other uncharged, and established a communication between the exterior coating of both, by means of a conducting body; and having then formed an arc from the interior coating of the one to that of the other, there

* Aloysii Galvani de Viribus Electricitatis in Motu musculari Commentarius, Mutinæ iterum editus, p. 29.

was an immediate transmission of the electricity with an explosion; and at the same time the flask which at first was uncharged became charged. If I formed an arc with my arms and hands, I experienced a considerable shock during the passage of the electric fluid. When I observed this effect, I conceived it was not contrary to the principles of philosophy to suppose that the one frog, in respect to the other, represented a Leyden flask, and at the same time acted the part of an arc.

XXI. Some, perhaps, will object to this analogy, that in the above experiments the flask destitute of electricity forms the arc; while, on the contrary, both flasks ought to be charged to represent properly the muscles and nerves of frogs, which both possess electricity. But the very same phænomenon is observed in two charged flasks, provided one of them is charged with more electricity than the other. Hence, in support of our analogy, we need only assume, that the quantity of electricity in the one frog is a little different from that of the other; a supposition which, in forming an hypothesis, the severest philosopher may allow. I shall say nothing of the great variety and connection of the elements of which animal bodies are composed, and which on this account require a difference in the quantity of the animal electricity. It appears by some late experiments of Valli*, that

* M. Valli Cinquième et Huitième Lettre sur l'Electricité Animale, dans Observations sur la Physique, par M. l'Abbé Rozier, tom. xlii. Paris, 1792.

animal

animal electricity is discharged in the same manner as that which is collected in non-conducting bodies. It is likewise proved, that the same arc and the same armatures, according to the various consensus and connection of the nerves, and according to the different positions in which they are applied, elicit a larger or smaller quantity of electricity, and sometimes none at all. Those indeed who consider these observations will hardly think it possible, when animal Leyden flasks consist of so many different parts, that there should not be some difference in the electricity collected. But there is no need of employing conjecture in regard to a point, which is confirmed not only in regard to animals, but in all conducting bodies, by the ingenious observations of Coulomb*; for it is fully established that the electric matter is communicated and accumulated different ways on the surface of conducting bodies. But if this phænomenon takes place in conducting bodies the parts of which are homogeneous, there is no reason to deny that it may appear in animals in which provident nature has so intermixed conducting with non-conducting parts, in order that the action of animal electricity might not be short and transient, but constant and durable.

XXII. But if the diversity in the structure of animal

* Recherches sur la Distribution de Fluide Electrique entre plusieurs Corps Conducteurs, et la Détermination de la Densité Electrique dans les différens Parties de la Surface de ces Corps. *Mem. de l'Acad. Royale des Sciences, An.* 1788.

bodies

bodies require that the force and power of the animal electricity, collected in the corresponding muscles, should be different, a singular agreement will appear between the phænomena of the Leyden flask and those of animal electricity. If an insulated person touch two flasks containing equal quantities of electricity, however great, he will experience no shock; but if one of the flasks contain a greater charge than the other, he will receive a shock according to the ratio of the difference of the electricity of the flasks. Though the frogs therefore represent two Leyden flasks, one of them may act the part of an arc, and produce an equilibrium, provided it be allowed that there is a difference between the quantities of the electricities collected. I have here endeavoured to establish the proposed analogy, not because I suppose the muscles to be so many Leyden flasks, such as they are exhibited to us by the ingenuity of philosophers, but in order to show that many phænomena are common to both; nor have I applied animal electricity to explain all the phænomena of muscular motion, with a view of obtaining applause from those who are zealous advocates for this theory. I must also observe, that if in the prosecution of this object I have met with any anomalies, I do not on that account despise the agreement of the laws of philosophy which have been established by so much labour. Several of the phænomena observed by Galvani and others have served me as a foundation for the proposed analogy, and induced me to extend, if possible, its boundaries. But going back to the

origin

origin of animal electricity, since it belongs to the subject, I shall here take a general view of the whole, and express the substance of it in a few corollaries.

XXIII. The corollaries I propose will follow the order of time in which they arose, and therefore will express the gradual improvement of animal electricity.

1. Animal electricity passes freely through bodies which possess nearly the same degree of conducting power, but it does not pass through non-conducting bodies.

2. It is affected by the obstacles which occur, not only in non-conducting but in conducting bodies, as well as by their varieties; and if these obstacles be numerous, its passage is stopped: but if it be possible to overcome them, the impediment causes it to make a more powerful effort to attain to a state of equilibrium. Hence, unlike armatures and arcs are of great effect in exciting a moderate degree of electricity, when the same electricity resists the power of a homogeneous metal.

3. Animal electricity obeys the law of equilibrium; for, when the muscles have been brought to a state of equilibrium with the corresponding nerves, no contractions are produced by the application of an arc; but if that which produces the equilibrium be removed, the contractions immediately take place.

4. Poisons,

4. Poisons, mephitic air, aëriform fluids, and condensed air, do not prevent animal electricity from being excited.

5. The influence of a vacuum on animal electricity is various. In dead animals, if kept a long time in vacuo, the animal electricity is weakened: in living animals it is considerably increased.

6. Though a vacuum does not prevent the electricity from being excited, it will not serve as a conductor of it when it has been excited: if an arc from the nerves to the muscles be intercepted by the smallest vacuum, no contractions take place.

7. As metallic armatures are of great effect in attracting and collecting artificial electricity, the case is the same with animal electricity; but care must be taken not to ascribe to them that electricity which the muscles naturally possess.

8. Though unlike armatures have a great effect in calling forth animal electricity, we have reason to conclude from several experiments, that they do not contain two kinds of electricity capable of producing muscular motion.

9. As natural electricity issues with great force from sharp-pointed bodies, and proceeds to them more readily than to others, the case is the same in regard to animal electricity, as it issues more readily from the pointed parts of the metallic arming applied to the nerves and muscles.

10. The

10. The nervo-electric fluid is propagated with that rapidity which is required in restoring to an equilibrium two opposite kinds of electricity.

11. The same conditions which cause two flasks to be discharged when an arc is established from the exterior coating of the one to the interior coating of the other, excite contractions in two frogs, when an arc is formed from the nerve of one to the muscles of the other.

12. The arc applied as above mentioned to the interior coating of two phials, and to the nerves of two frogs, seems to give more force to the proposed analogy; for the electric explosions have a great similarity to the muscular motions excited in the frogs.

I might have enlarged the number of these corollaries, had not the well-known fate of various opinions, now consigned to oblivion, rendered me more timid in hazarding conjectures. I, however, did not allow myself to think that I ought so far to give way to my timidity as to check the spirit of inquiry, or to abandon the hope of one day attaining to the truth. But it would be unreasonable to expect in animal electricity, which is yet in its infancy, that precision and those satisfactory results which can be the work only of time, and of the continued labour of philosophers.

CONCLUSION.

I PUBLISH my experiments respecting muscular contractions produced by one metal with the greater confidence, as they were repeated different ways by the celebrated Humboldt, who has adopted my opinion. In his work on Galvanism, under the head which he entitles " Answer to the Objections made by Volta to the Experiments of Aldini," he says, " J. Aldini of Bologna invented a very ingenious apparatus, by means of which he was enabled to refute the supposition of Professor Volta. For this purpose he had recourse to mercury: every thing relating to his experiments is very well represented in the plate which accompanies his memoir. Volta, in reply to these experiments, observes, that they can impose only on those who have not thoroughly examined them. He denies the facts related by Aldini, and persists in his opinion that the phænomena of Galvanism may all be referred to the laws of heterogeneity. In regard to the experiments made with mercury, he says that there is a great difference between the surface of this metal and the interior of its mass; because the surface becomes oxidated by the contact of the atmospheric air; that consequently, in the experiment of Aldini, the conducting arc is not really but apparently homogeneous, the organs being immersed to different depths in the metal: besides, that the mercury in these experiments produces a

shock;

shock; and that, as this shock is not the same at both extremities of the arc, the result is an unequal development of electricity. Volta, therefore, opposes to the phænomena described by Aldini nothing but hypothesis. We might reply in the same manner; but as it is much better in philosophical disputes to have recourse to experiments, I undertook some researches for the purpose of removing all doubt in regard to this subject.

"I purified mercury by all the known means employed for that purpose. I strained it several times through a piece of chamois leather; I washed it with water and soap, with vinegar and with alcohol. It appeared to me that it contained neither lead nor tin; and that it was free from oily particles and dust: it was perfectly fluid, and divided itself into small, very round globules, which did not adhere to each other, and which left behind them no traces. Its surface was as brilliant as that of glass, without any pellicle or spot; and a small quantity of it being stirred in a mortar with water, did not communicate to it any sensible colour. It dissolved in nitric acid without sensible effervescence, and without giving any precipitate. A large quantity of mercury thus purified was poured into three porcelain vessels; and as I was aware that, if I performed several experiments with the same quantity of mercury, it might be objected, that the metal had contracted some impurity by the mere contact of the animal substances, I made only one experiment with the mercury in each of the vessels.

 "I pre-

"I prepared several legs of frogs in such a manner that a portion of the crural nerve and a part of the muscle of the same length were left dependent. I placed a glass tube in a horizontal direction above a vessel containing mercury; made fast to it two silk threads, and suspended from it the thigh of the leg in such a manner that the nerve and muscle could be made to descend at pleasure. I brought the leg of the frog within two lines of the vessel, and then lengthened the thread till the nerve touched the surface of the metal. No contractions took place; but as soon as the nerve also was brought into contact with the metal, by lengthening the silk thread, the whole limb experienced a convulsive shock. I repeated this experiment with the same precaution, employing the two other vessels, and the same results were obtained.

"The muscle and nerve touched the mercury only at the surface; they were in no manner immersed in the metal; and care had been taken to lower them so gently, that it was impossible to suspect there could be any impulse, as in the experiment where Aldini had caused the mercury to flow down by employing a vessel shaped somewhat like a syphon."

APPENDIX.

No. I.

An Account of the Experiments performed by J. ALDINI *on the Body of a Malefactor executed at Newgate Jan. 17th* 1803.

INTRODUCTION.

THE unenlightened part of mankind are apt to entertain a prejudice against those, however laudable their motives, who attempt to perform experiments on dead subjects; and the vulgar in general even attach a sort of odium to the common practice of anatomical dissection. It is, however, an incontrovertible fact, that such researches in modern times have proved a source of the most valuable information, in regard to points highly interesting to the knowledge of the human frame, and have contributed in an eminent degree to the improvement of physiology and anatomy. Enlightened legislators have been sensible of this truth; and therefore it has been wisely ordained by the British laws, which are founded on the basis of humanity aud public benefit, that the bodies of those who during life violated one of the most sacred rights of mankind, should after execution be devoted to a purpose which might make some atonement for their crime, by rendering

rendering their remains beneficial to that society which they offended.

In consequence of this regulation, I lately had an opportunity of performing some new experiments, the principal object of which was to ascertain what opinion ought to be formed of Galvanism as a mean of excitement in cases of asphyxia and suspended animation. The power which exists in the muscular fibre of animal bodies some time after all other signs of vitality have disappeared, had before been examined according to the illustrious Haller's doctrine of irritability; but it appeared to me that muscular action might be excited in a much more efficacious manner by the power of the Galvanic apparatus.

In performing these experiments, I had another object in view. Being favoured with the assistance and support of gentlemen eminently well skilled in the art of dissection, I proposed, when the body should be opened, to perform some new experiments which I never before attempted, and to confirm others which I had made above a year ago on the bodies of two robbers decapitated at Bologna.

To enlarge on the utility of such researches, or to point out the advantages which may result from them, 'is not my object at present. I shall here only observe, that as the bodies of valuable members of society are often found under similar circumstances, and with the same symptoms as those observed

observed on executed criminals; by subjecting the latter to proper experiments, some speedier and more efficacious means than any hitherto known, of giving relief in such cases, may, perhaps, be discovered. In a commercial and maritime country like Britain, where so many persons, in consequence of their occupations at sea, on canals, rivers, and in mines, are exposed to drowning, suffocation, and other accidents, this object is of the utmost importance in a public view, and is entitled to every encouragement.

Forster, on whose body these experiments were performed, was twenty-six years of age, seemed to have been of a strong, vigorous constitution, and was executed at Newgate on the 17th of January 1803. The body was exposed for a whole hour in a temperature two degrees below the freezing point of Fahrenheit's thermometer; at the end of which long interval it was conveyed to a house not far distant, and, in pursuance of the sentence, was delivered to the College of Surgeons. Mr. Keate, master of that respectable society, having been so kind as to place it under my direction, I readily embraced that opportunity of subjecting it to the Galvanic stimulus, which had never before been tried on persons put to death in a similar manner: and the result of my experiments I now take the liberty of submitting to the public.

Before I conclude this short introduction, I consider it as my duty to acknowledge my obligations to Mr. Carpue, lecturer on anatomy, and Mr. Hutchins, a medical pupil, for the

the assistance they afforded me in the dissection. I was also much indebted to Mr. CUTHBERTSON, an eminent mathematical instrument maker, who directed and arranged the Galvanic apparatus. Encouraged by the aid of these gentlemen, and the polite attention of Mr. KEATE, I attempted a series of experiments, of which the following is a brief account.

EXPE-

EXPERIMENT I.

ONE arc being applied to the mouth, and another to the ear, wetted with a solution of muriate of soda (common salt), Galvanism was communicated by means of three troughs combined together, each of which contained forty plates of zinc, and as many of copper. On the first application of the arcs the jaw began to quiver, the adjoining muscles were horribly contorted, and the left eye actually opened.

EXPERIMENT II.

On applying the arc to both ears, a motion of the head was manifested, and a convulsive action of all the muscles of the face: the lips and eyelids were also evidently affected; but the action seemed much increased by making one extremity of the arc to communicate with the nostrils, the other continuing in one ear.

EXPERIMENT III.

The conductors being applied to the ear, and to the rectum, excited in the muscles contractions much stronger than in the preceding experiments. The action even of those mus-

cles furthest distant from the points of contact with the arc was so much increased as almost to give an appearance of re-animation.

EXPERIMENT IV.

In this state, wishing to try the power of ordinary stimulants, I applied volatile alkali to the nostrils and to the mouth, but without the least sensible action: on applying Galvanism great action was constantly produced. I then administered the Galvanic stimulus and volatile alkali together; the convulsions appeared to be much increased by this combination, and extended from the muscles of the head, face, and neck, as far as the deltoid. The effect in this case surpassed our most sanguine expectations, and vitality might, perhaps, have been restored, if many circumstances had not rendered it impossible.

EXPERIMENT V.

I next extended the arc from one ear to the biceps flexor cubiti, the fibres of which had been laid bare by dissection. This produced violent convulsions in all the muscles of the arm, and especially in the biceps and the coraco brachialis even without the intervention of salt water.

EXPE-

EXPERIMENT VI.

An incision having been made in the wrist, among the small filaments of the nerves and cellular membrane, on bringing the arc into contact with this part, a very strong action of the muscles of the fore-arm and hand was immediately perceived. In this, as in the last experiment, the animal moisture was sufficient to conduct the Galvanic stimulus without the intervention of salt water.

EXPERIMENT VII.

The short muscles of the thumb were dissected, and submitted to the action of the Galvanic apparatus, which induced a forcible effort to clench the hand.

EXPERIMENT VIII.

The effects of Galvanism in this experiment were compared with those of other stimulants. For this purpose, the point of the scalpel was applied to the fibres, and even introduced into the substance of the biceps flexor cubiti without producing the slightest motion. The same result was obtained from the use of caustic volatile alkali and concentrated

sulphuric acid. The latter even corroded the muscle, without bringing it into action.

EXPERIMENT IX.

Having opened the thorax and the pericardium, exposing the heart *in situ*, I endeavoured to excite action in the ventricles, but without success. The arc was first applied upon the surface, then in the substance of the fibres, to the carneæ columnæ, to the septum ventriculorum, and lastly, in the course of the nerves by the coronary arteries, even with salt water interposed, but without the slightest visible action being induced.

EXPERIMENT X.

In this experiment the arc was conveyed to the right auricle, and produced a considerable contraction, without the intervention of salt water, but especially in that part called the appendix auricularis: in the left auricle scarcely any action was exhibited.

EXPERIMENT XI.

Conductors being applied from the spinal marrow to the fibres of the biceps flexor cubiti, the gluteus maximus, and the gastrocnemius, separately, no considerable action in the muscles of the arm and leg was produced.

EXPE-

EXPERIMENT XII.

The sciatic nerve being exposed between the great trochanter of the femur and the tuberosity of the ischium, and the arc being established from the spinal marrow to the nerve divested of its theca, we observed, to our astonishment, that no contraction whatever ensued in the muscles, although salt water was used at both extremities of the arc. But the conductor being made to communicate with the fibres of the muscles and the cellular membrane, as strong an action as before was manifested.

EXPERIMENT XIII.

By making the arc to communicate with the sciatic nerve and the gastrocnemius muscle, a very feeble action was produced in the latter.

EXPERIMENT XIV.

Conductors being applied from the sciatic to the peronæal nerve, scarcely any motion was excited in the muscles.

EXPERIMENT XV.

The sciatic nerve being divided about the middle of the thigh, on applying the conductors from the biceps flexor cruris to

to the gastrocnemius, there ensued a powerful contraction of both. I must here observe that the muscles continued excitable for seven hours and a half after the execution. The troughs were frequently renewed, yet towards the close they were very much exhausted. No doubt, with a stronger apparatus we might have observed muscular action much longer; for, after the experiments had been continued for three or four hours, the power of a single trough was not sufficient to excite the action of the muscles: the assistance of a more powerful apparatus was required. This shows that such a long series of experiments could not have been performed by the simple application of metallic coatings. I am of opinion that, in general, these coatings, invented in the first instance by Galvani, are passive. They serve merely to conduct the fluid pre-existent in the animal system; whereas, with the Galvanic batteries of Volta, the muscles are excited to action by the influence of the apparatus itself.

FROM the above experiments there is reason to conclude:

I.

That Galvanism exerts a considerable power over the nervous and muscular systems, and operates universally on the whole of the animal œconomy.

II. That

II.

That the power of Galvanism, as a stimulant, is stronger than any mechanical action whatever.

III.

That the effects of Galvanism on the human frame differ from those produced by electricity communicated with common electrical machines.

IV.

That Galvanism, whether administered by means of troughs, or piles, differs in its effects from those produced by the simple metallic coatings employed by Galvani.

V.

That when the surfaces of the nerves and muscles are armed with metallic coatings, the influence of the Galvanic batteries is conveyed to a greater number of points, and acts with considerably more force in producing contractions of the muscular fibre.

VI. That

VI.

That the action of Galvanism on the heart is different from that on other muscles. For, when the heart is no longer susceptible of Galvanic influence, the other muscles remain still excitable for a certain time. It is also remarkable that the action produced by Galvanism on the auricles is different from that produced on the ventricles of the heart, as is demonstrated in Experiment the tenth.

VII.

That Galvanism affords very powerful means of resuscitation in cases of suspended animation under common circumstances. The remedies already adopted in asphyxia, drowning, &c. when combined with the influence of Galvanism, will produce much greater effect than either of them separately.

TO conclude this subject, it may be acceptable to the reader to have a short but accurate account of the appearances exhibited on the dissection of the body, which was performed with the greatest care and precision by Mr. Carpue.

"The

"The blood in the head was not extravasated, but "several vessels were prodigiously swelled, and the lungs "entirely deprived of air; there was a great inflamma- "tion in the intestines, and the bladder was fully distended "with urine. In general, upon viewing the body, it ap- "peared that death had been immediately produced by a "real suffocation."

It may be observed, if credit can be given to some loose reports, which hitherto it has not been in our power to substantiate, that after this man had been for some time suspended, means were employed with a view to put an end to his sufferings.

From the preceding narrative it will be easily perceived, that our object in applying the treatment here described was not to produce re-animation, but merely to obtain a practical knowledge how far Galvanism might be employed as an auxiliary to other means in attempts to revive persons under similar circumstances.

In cases when suspended animation has been produced by natural causes, it is found that the pulsations of the heart and arteries become totally imperceptible; therefore, when it is to be restored, it is necessary to re-establish the circulation throughout the whole system. But this cannot be done without re-establishing also the muscular powers which have

been suspended, and to these the application of Galvanism gives new energy.

I am far from wishing to raise any objections against the administration of the other remedies which are already known, and which have long been used. I would only recommend Galvanism as the most powerful mean hitherto discovered of *assisting* and increasing the efficacy of every other stimulant.

Volatile alkali, as already observed, produced no effect whatever on the body when applied alone; but, being used conjointly with Galvanism, the power of the latter over the nervous and muscular system was greatly increased: nay, it is possible that volatile alkali, owing to its active powers alone, might convey the Galvanic fluid to the brain with greater facility, by which means its action would become much more powerful in cases of suspended animation. The well known method of injecting atmospheric air ought not to be neglected; but here, likewise, in order that the lungs may be prepared for its reception, it would be proper previously to use Galvanism, to excite the muscular action, and to assist the whole animal system to resume its vital functions. Under this view, the experiments of which I have just given an account, may be of great public utility.

It is with heartfelt gratitude that I recall to mind the politeness and lively interest shown by the members of the College

College of Surgeons in the prosecution of these experiments. Mr. Keate, the master, in particular proposed to make comparative experiments on animals, in order to give support to the deductions resulting from those on the human body. Mr. Blicke observed that on similar occasions it would be proper to immerse the body in a warm salt bath, in order to ascertain how far it might promote the action of Galvanism on the whole surface of the body. Dr. Pearson recommended oxygen gas to be substituted instead of the atmospheric air blown into the lungs. It gives me great pleasure to have an opportunity of communicating these observations to the public, in justice to the eminent characters who suggested them, and as an inducement to physiologists not to overlook the minutest circumstance which may tend to improve experiments that promise so greatly to relieve the sufferings of mankind.

No. II.

Report presented to the Class of the Exact Sciences of the Academy of Turin, 15th August 1802, in regard to the Galvanic Experiments made by C. VASSALI-EANDI, GIULIO, *and* ROSSI, *on the 10th and 14th of the same Month, on the Bodies of three Men a short Time after their Decapitation. By* C. GIULIO.

THE First Consul, in a letter to Chaptal, in which he announced to that minister the two prizes he had founded to encourage philosophers to make new researches in regard to Galvanism, says, "Galvanism, in my opinion, will lead to great discoveries." This observation was just and profound: great discoveries have already been made; Galvani and Volta have immortalized their names, and several celebrated philosophers and physiologists have rendered themselves illustrious in this branch of science, so abundant in astonishing phænomena: yet it is only in its infancy, and there can be no doubt that many important discoveries still remain to be made.

Vassali, Rossi, and myself, have for several years been employed in researches on this subject. While the first examined the Galvanic fluid in every point of view, for the purpose of illustrating its nature by means of a great number of ingenious experiments, performed with that care and exactness for which he is distinguished, Rossi and I attempted

to

to explain the action of the Galvanic fluid on the different organs of the animal œconomy.

Sometimes I was obliged to interrupt my researches by unfortunate circumstances, and at others by my administrative functions: but I have now resumed them; and though success has not yet crowned our efforts by any brilliant discovery, we trust, and with confidence, that we shall be able to add some valuable facts to the history of the animal œconomy; to rectify others; to confirm facts already received; and to extend the domain of an inexhaustible agent fertile in wonders.

Volta had announced that the involuntary organs, such as the heart, the stomach, the intestines, the bladder and vessels, are insensible to the Galvanic action*: but we have fully refuted this great physiological error. Unfortunately, however, the Latin memoir containing the decisive experiments which we made on cold-blooded and warm-blooded animals in 1792, presented to the Academy soon after, and which, according to Sue, in his History of Galvanism †, "are curious, and contain very interesting observations," did not appear till 1801,

* Mezzini, Volta, Valli, Klein, Pfaff, Berhends, have denied that the heart could be moved by the Galvanic fluid. *Hist. du Galvanisme*, part i. p. 145. Bichat could obtain no contractions either in the heart of man or that of the dog. See *Rêcherches Physiologiques sur la Vie et la Mort.*

† Towards the end of the First Part.

when

when it was printed in the last volume of the Transactions of the Academy.

In that interval Grapengiesser found, as we had done, that Galvanism, by means of zinc and silver*, has an influence on the peristaltic motion. Humboldt ascertained the Galvanic action on the hearts of frogs, lizards, toads, and fishes. Smuch observed the excitability of the heart by the Galvanic fluid; and Fowler changed the pulsations of the heart without the immediate application to it of armatures, and only by adapting them in warm-blooded animals to the recurrent nerve by means of the sympathetic†.

It is chiefly in regard to the experiments of these learned Germans that the historian of Galvanism states‡, that the involuntary vermicular motion of the intestines, according to the acknowledgment of all physiologists, obeys metallic irritation; whence it follows, says he, that the Italian philosophers have advanced an error when they said that Galvanism exercises no action but on the muscles, which depend on the will. As an accurate and impartial historian, how can Sue accuse the Italian philosophers indiscriminately of such an error, since he had our memoir before him when employed on the second volume of his History of Galvanism, and since he gave a short account of my experiments in his first volume? Nay,

* See *Histoire du Galvanifme*, vol. ii. p. 81.
† Ibid. vol. ii. p. 84. ‡ Vol. ii. p. 83.

I gave

I gave an account of my experiments in a small work published in Italian in 1792. But as Italian works are not much read in France, and were less so at that period, I should not have reproached C. Sue with this act of injustice, and his incorrectness in regard to the Italian philosophers, had not my Latin memoir been known to him, as it had appeared in the Transactions of the Academy.

Though we made a great many experiments before we attempted to combat a philosopher so justly celebrated as Volta, and to establish the influence of Galvanism on the involuntary organs; and though Grapengiesser, Humboldt, Smuch, Fowler, &c. ascertained this influence in certain cold-blooded and even warm-blooded animals; an object of so much importance to physiology required to be extended and confirmed, especially in man, by new experiments. We have been the more sensible of the necessity of establishing this fact in an incontestable manner, either in regard to the involuntary organs in general, or more particularly the heart, as the celebrated Aldini, professor at Bologna, in an Italian work replete with new facts and valuable experiments made on the bodies of decapitated criminals, has been obliged to acknowledge that he was not able to obtain any contraction in that organ by means of the electro-motor of Volta, which is so powerful.

We shall give an account, in particular memoirs, of the experiments we have already made, and of those which we

propose

propose to perform. In regard to the stomach, the large and the small intestines, and the bladder, we shall say only, in a general manner, that by armature of the different nervous branches we obtained contractions analogous to those described in regard to animals. The Galvanic action on the heart and arteries is the object of the present paper, as it is of the utmost importance to physiology, and deserves, under every point of view, to excite our attention and occupy our reflections.

Our experiments on the different parts of the head and trunk of the decapitated criminals were begun, on the 10th of August, in a hall of the large hospital of St. John, and resumed and continued yesterday in the anatomical theatre of the university, before a great number of spectators.

We tried the influence of Galvanism on the heart in three different ways:

1st, In arming the spinal marrow by means of a cylinder of lead introduced into the canal of the cervical vertebræ, and then conveying one extremity of a silver arc over the surface of the heart, and the other to the arming of the spinal marrow. The heart of the first individual subjected to our experiments immediately exhibited very visible and very strong contractions. These experiments were made without the intervention of any kind of pile, and without any armature applied to the heart. It is very remarkable, that when the

former

former is touched first, and then the arming and spinal marrow, the contractions of the heart which follow are more instantaneous, and stronger, than when the arming of the spinal marrow is first touched, and then the heart. In a memoir on Galvanism, read in the last public sitting of the academy, I gave an account of a great number of experiments, made especially on frogs, which exhibited a similar phænomenon. In these animals I observed, a great number of times, that when the arming of the crural nerves was touched first, and then the muscles of the thigh, there were no contractions, or the contractions were exceedingly weak; and, on the other hand, that when the muscles of the thighs were first touched, and then the arming of the crural nerves, as long as the least vitality remained in the organs, the contractions of the muscles were constant and violent. In the memoir already mentioned I have endeavoured to account for this phænomenon, to which I shall recur, when it has been ascertained by a sufficient number of trials, that it is as general in men as I found it in frogs and other cold-blooded animals.

The second manner in which we tried the influence of Galvanism on the heart was by arming the par vagum and the great sympathetic nerve. The object of these experiments will be readily comprehended by anatomists acquainted with the details of neurology. In these, as well as in the first and other experiments where we armed the cardiac nerves themselves, we obtained contractions in the heart. In this, as in the

former case, the contractions took place when the heart was first touched, and then the arming of the nerves, were much stronger than when the arming of the nerves was touched first, and then the heart. In this method we even observed that the Galvanic experiments sometimes failed.

The third kind of experiments on the heart were performed by means of the pile. The pile employed on the 10th of August, for the experiments on the first decapitated criminal, was composed of fifty plates of silver and as many of zinc, with pasteboard moistened by a strong solution of muriate of soda. The silver was mixed with a tenth part of copper. This is the proportion which we found most favourable to the intensity of the signs of Galvanism:

		Metre.
The diameter of the silver plates was	-	0·036
Their thickness	- - -	0·0015

The dimensions of the pieces of pasteboard were the same.

		Metre.
The diameter of the zinc plates was	-	0·042
Their thickness	- - -	0·0035

The pile employed for the experiments on the 15th of August was composed of fifty plates of pure silver, and twice that

that number of plates of zinc and pieces of pasteboard; the latter moistened in a solution of muriate of soda.

		Metre.
The diameter of the silver plates was	-	0·038
Their thickness	- - -	0·001

The dimensions of the pieces of pasteboard were the same.

		Metre.
The diameter of the zinc plates was	-	0·04
Their thickness	- - -	0·001

By making the negative extremity of the pile to communicate, by means of respective conductors, with the spinal marrow, or merely with the muscles of the back or breast, laid bare, and the positive extremity immediately with the heart, instantaneous and violent contractions were obtained; and the contractions were produced also when the heart was made to communicate with the negative extremity of the pile, and the spinal marrow with the positive extremity.

We shall observe, in regard to contractions of the heart, that of all its parts the apex is the most susceptible of motion, and the most sensible to the Galvanic influence: we must observe also, that the contractions produced by communication with the pile were not only strong, but that they continued a long time even after the communication was destroyed.

 A very

A very remarkable circumstance is, that the heart, which of all the muscles retains longest, in general, its contractility in regard to mechanical stimulants, is the first to become insensible to the Galvanic influence. The muscles of the arms, and those of the back and breast, continue to be excitable by Galvanism for whole hours; and the heart had lost its excitability about forty minutes after death.

The experiments made yesterday in the anatomical theatre exhibited nearly the same results in regard to the heart as those already mentioned. The great arteries, such as the aorta and some of its branches, being injected with water raised nearly to the same temperament as that of the blood in the living individual, and subjected to the Galvanic action, exhibited contractions. But it is probable that they will appear stronger when trials of this kind shall be made on bodies endowed with a higher degree of vitality than those of yesterday, and when the interval between the period of decapitation and that of the experiments shall be less. With this view, indeed, we have provided a hall much nearer to the place of execution; for the results which we obtained in the man decapitated on the 10th of August, in which case the experiments were begun five minutes after the decapitation, were all comparatively more striking, and stronger, than those obtained in the experiments of yesterday, which were begun more than twenty minutes after decapitation, and which were performed, as appears, on bodies endowed with a much less degree of vitality.

In the experiments made on the arteries, we armed the nervous plexuses, which envelop the trunks of the cœliac and mesenteric arteries, several branches of which are even interwoven around the aorta: a communication was established between the positive or negative extremity of the pile and the aortic artery itself. It was by these means that we obtained visible contractions.

If the effects of Galvanism on arterial contractions be constant, as I suppose them to be, all those discussions which have been agitated so long, and with so much violence, in regard to the irritability of the arteries, which does not manifest itself by the action of different mechanical and chemical stimulants, will at length be terminated in a positive and irrefragable manner; all doubts will at length be removed; and we shall be indebted to the Galvanic fluid, which is the most energetic of all agents applied to the animal fibre, for having fixed the opinions of physiologists on a point of so much importance to the animal œconomy.

Whence comes it that Aldini, even with the help of the most powerful electro-motors, was not able to obtain contractions in the heart of man, which we so evidently obtained by the same means which always withstood his efforts? How happens it that we obtained contractions by means much weaker?

The first experiments of Aldini on the human heart were begun

begun an hour and a half after death*. The trunk had been exposed a long time to the open air, the temperature of which was no more than + 2. It is probable that the cold, and the long interval between the period of death and that of the experiment, had already annihilated the irritability of the heart†. In the fifty-third experiment, the heart of another executed criminal constantly remained motionless and insensible to the Galvanic current. But in this experiment, before trying the heart, a considerable time was employed in making trials on the voluntary organs, the sensibility of which to Galvanism had already been acknowledged. But the very reverse of this method ought to be followed; for I will here repeat, that excitability, by means of the Galvanic fluid, is extinguished in the heart a long time before it becomes extinct in the voluntary muscles. This is so certain, that while no part of the heart, tried externally and internally, presented any sign of contractions, the diaphragm, and the muscles of the upper and lower extremities, gave very strong ones.

* Saggio di Sperienze sul Galvanismo di Giovani Aldini; Bolonia 1802, p. 14, esp. 28.

† If the celebrated Bichat failed in his experiments on the human heart, as well as Aldini, it was, perhaps, owing to the same causes. The temperature was cold, and the interval between the time of execution and that of the experiment too long. "I was authorized," says Bichat, "in the winter of the year 7, to make various trials on the bodies of unfortunate persons who had been guillotined. I had them at my disposal from thirty to forty minutes after execution. It was always impossible for me to produce the least motion by arming either the spinal marrow and the heart, or the latter organ and the nerves which it receives from the ganglions by the sympathetic, or from the brain by the par vagum.

In

In our experiments which were begun five minutes after death, the heart ceased to be sensible to the Galvanic agent about the fortieth minute; and this was the case in the temperature of + 25; while the voluntary muscles retained their Galvanic excitability for hours. In other experiments made by Aldini, the contractility of the voluntary muscles existed three hours, and even five hours, after death.

In the oxen subjected to Galvanic experiments by Aldini, the excitability of the heart must have been extinguished sooner, since the action of the Galvanic fluid of the pile produced no contractions, though applied immediately after death.

If contractions were observed in the voluntary muscles under the same circumstances, it was because these muscles, which lose much sooner than the heart their excitability in regard to mechanical stimulants, retain it much longer than that organ in regard to the Galvanic agent. What then is the cause of this diversity, which seems contrary to every analogy, and which, however, is proved by facts? It is still involved in much obscurity: but it is not yet time to tear the dark veil which conceals it; we are not yet enlightened by a sufficient number of facts; and the few scattered data which we have been able to collect, cannot yet be connected in a manner capable of encouraging us to attempt to rend the veil at present.

We shall not here speak of the astonishment with which the spectators were struck when they saw the contractions of the frontal muscles, those of the eye-lids, the face, the lower jaw, and the tongue; when they beheld the convulsions of the muscles of the arms, the breast, and of the back, which raised the trunk some inches from the table; the contractions of the pectoral muscles, and the exterior and interior intercostal muscles, which diminished the intervals between all the ribs, and made them approach each other with violence, raising the inferior ones towards the superior, and the latter towards the first rib and the clavicle; the contractions of the arms, which, when the uncovered biceps muscle was touched, as well as its tendon, were so speedy and violent, that complete flexion of the fore-arm on the arm took place, and that the hand raised weights of some pounds fifty minutes after decapitation. Similar experiments may be seen in the work of Aldini: our object in this report was merely to speak of the Galvanic influence on the heart and arteries of man, which had not yet been observed.

These new and important results, which we obtained in regard to the heart and arteries of man, will be confirmed by other trials. We shall repeat our experiments as soon as an opportunity occurs, and we shall take care to give you an early account of the most remarkable observations we shall make.

No. III.

No. III.

Account of an experiment made at Calais, on the transmission of Galvanism through an arm of the sea. By J. ALDINI.

THE experiments made at the Lake of Geneva by Mr. de Luc and his brother, and those made in England on the banks of the Thames, by which it has been proved that common electricity is susceptible of transmission through a considerable space of water, induced me to try some analogous experiments in regard to Galvanism by endeavouring to make it traverse a certain extent of sea.

Some philosophers, to whom I communicated this project, seemed to entertain doubts on the subject, as they conceived that a considerable extent of sea water might perhaps destroy or impede the action of the Galvanic fluid. My late passage at Calais afforded me an opportunity of removing all these doubts by an experiment which was attended with complete success.

 M. Sept-

M. Sept-Fontaines, distinguished by his philosophical knowledge, was desirous of assisting me in my proposed researches; M. Cheely, chemist of the military hospital, prepared the necessary instruments; and M. Debaudre, the port engineer, conducted the arrangement of the Galvanic arcs.

On the 27th of February, the sky being serene, and the sea calm, every thing seemed to be favourable for the experiment. A gentle south-west wind prevailed at the time; the temperature of the water of the sea was 47·4° of Fahrenheit, that of the atmosphere 49·4°, and the barometer stood at 30·37 inches.

Fort Rouge and the West Mole afforded me two fixed points proper for my purpose. A Galvanic pile consisting of eighty plates of silver and zinc was constructed on the West Mole on an insulated stool, and the animals destined to be exposed to the Galvanic action were placed at Fort Rouge. The Galvanic chain was composed of the arm of the sea which separates Fort Rouge from the Western Mole, and of three wires disposed in the following manner.

The first wire proceeded from the base of the pile, and, being supported by an insulator, fell vertically into the sea to the depth of about three fathoms.

The second wire, insulated in the same manner, proceeded from the summit of the pile, and was conveyed in a horizon-

tal

tal direction at the height of from six to nine feet above the surface of the sea, as far as the platform of Fort Rouge.

A third wire, also insulated, and placed at one corner of the platform, descended perpendicularly into the sea in the same manner as the first.

When this arrangement was made, if a person on the platform touched the extremities of the second and third wires, and thus completed the Galvanic circle, he always experienced a shock; and when animals recently killed were substituted in the room of the person, they were thrown into strong convulsions. We therefore concluded that the portion of sea water between the pile and the animal subjected to its action formed a part of the Galvanic circle: such was the consequence we thought ourselves authorised to deduce from this experiment. The breadth of the water was about 200 feet.

I must freely confess, that in repeating these experiments, we found, that to receive the shock, it was not absolutely necessary that the person should hold in his hands the two conductors, and that it was sufficient to touch the wire alone which proceeded from the summit of the pile. This apparent anomaly deranged at first the result of my researches; and we suspected that the shocks before received had been transmitted without the intervention of the water of the sea. It was therefore necessary that this doubt should be cleared up by new observations.

 I tried

I tried separately, on the platform, the action of the two conducting wires, and found that by touching the wire which fell into the sea no shock was produced. I then took in my hand the other wire, which proceeded from the summit of the pile; and having thus brought its action into equilibrium, I experienced a shock: which shows that the Galvanic fluid took its course from the bottom of the pile traversing the sea. M. Sept-Fontaines proposed that we should lower to the level of the sea the wire which proceeded from the summit of the pile, and which was extended to Fort Rouge. The action of the Galvanism was then checked, but was immediately restored by placing the conductor in its former position. Thus, notwithstanding the large extent of water by which the metallic conductors were separated; and notwithstanding the agitation produced by the sea, the Galvanism found no obstacle to its propagation, and pursued its usual direction.

Hence it may be readily perceived, that though the experiments here described are analogous to those formerly made with the Leyden flask on lakes and rivers, they are new of their kind, and may contribute to establish the similarity between the properties of common electricity and of Galvanism. I will even venture to assert, that these experiments, if pursued and varied, may lead to some interesting discoveries in natural philosophy.

After making these observations at Fort Rouge, I repaired in

in company with M. Sept-Fontaines to the West Mole, to try the power of the pile unconnected with the sea. Having formed an arc, we found that the action of the pile, in this case, was stronger; which induced us to conclude, that the Galvanic power in traversing the sea had been in some measure weakened. There is reason to suppose, that by transmitting the Galvanic influence gradually to greater distances through the sea, the point of the minimum of its action, that is, a distance at which it will no longer be sensible, may be discovered. This distance remains to be determined, and also the difference between the propagation of common electricity in fresh water, and that of Galvanism in salt water.

I observed that the sea shore, still moist after the water retires in consequence of the reflux, is endowed with the power of conveying Galvanism to very great distances. I made several experiments on this subject with M. Bastide, physician of Calais, who acknowledged that he had received very strong shocks, the effects of which were sensible the day following.

THE END.

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